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AND

ANALECTIC JOURNAL.

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Auctoribus suus constat honos.—BACON.

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PREFACE.

When a new periodical work is given to the public, its readers expect, as a necessary preliminary, a statement of the principles on which it is to be conducted. Such an expectation is so reasonable, that the Editors of this Journal will not only endeavour to comply with it, but they will also explain the motives by which they have been actuated in undertaking a most arduous and responsible engagement.

On the present occasion, indeed, this must appear to be necessary from several considerations. There are already so many journals in circulation, that an additional one will probably be regarded as superfluous. There are also such rival schools, and parties, and doctrines in medicine, that we may be suspected of sinister designs and concealed partialities. There have been, moreover, comparatively speaking, so few original ideas in the various publications of successive years, that it may be supposed impossible to fill up the pages of a professed review with matter either of an interesting or useful character. To all such misapprehensions we trust that a candid avowal of the motives which have governed us in commencing this work, and of the principles by which we shall abide in continuing it, will prove a sufficient reply.

The demand for medical publications is by no means inconsiderable; and, reciprocally, as a plentiful market attracts a crowd of purchasers, so, in our profession, will a prolific press excite the intellectual appetite. We believe, therefore, that although other journals have been established, the one

in which we are now engaged will neither interfere with them, nor injure the cause of medical science. On the contrary, if our undertaking prove at all successful, it will have a tendency to excite others by a salutary competition, while, at the same time, it may convey additional information to the mass of readers.

But there are positive reasons which have more immediately influenced us to commence this work. The very extensive regions through which the members of our profession are distributed, require some general vehicle of intelligence to convey to them the knowledge of the day. Such periodical works as have hitherto been published in this country do not appear to be fully calculated to answer this important purpose. They are generally connected with local circumstances, in such a manner, as to render it impossible to derive from them a complete knowledge of the actual state of medicine, at any particular period. However instructive they may be in regard to the subjects of which they treat, their scope is too limited to comprise that general information upon all medical subjects which the practitioners of our country appear to require. Actuated by the hope of being able to supply this deficiency, we have commenced the present work, on a plan which is so general as to embrace every subject of professional import, at the same time that it is sufficiently comprehensive to admit of much selection in a moderate space. As the conductors of the *Archives Générales de Medecine* have already observed, we desire to publish a journal "*qui conçu et executé sur un plan plus vaste que la plupart des enterprises du meme genre, réunisse les avantages offerts par chacune d'elles en particulier.*"

The plan of this work can be very concisely explained. It will be chiefly composed of reviews of a critical and analytic character; which will be followed by a short original department for the reception of interesting papers, and by copious selections from every source of medical intelligence.

In the reviews, which we shall attempt to make as instructive as possible, will be given the spirit and substance of every valuable publication in medicine and its collateral branches.

Into the original department no communication will be admitted which is not important, and derived from the most respectable and authentic sources. It has been established only to comply with the solicitations of some gentlemen of high standing in the profession, who intend to honour it with their contributions.

We would by no means wish, however, to convey the idea that information of the practical observations and improvements of our friends will not be acceptable. On the contrary, we shall always be pleased to notice every thing of that kind with which we may be favoured, in a conspicuous manner, under the head of "Intelligence."

The Analecta will be sufficiently varied and condensed to comprise notices of every subject that can prove either useful or interesting to the practitioner. In short, we shall endeavour to employ, in the construction of this work, all the materials of professional knowledge which may be afloat in the publications of the day.

The reviews will be conducted with that spirit of liberality which should always characterise such productions; and we shall use our best efforts that they may not be found deficient either in discernment or accuracy. If occasionally it shall

become necessary to controvert the opinions of authors, or to impugn the correctness of their statements, as much lenity as may be consistent with public utility will be exhibited towards them. The book, and not the author, will be made the subject of remark.

———*Licuit, semperque licebit*
Parcere personis, dicere de vitiis.

But we must do justice to our readers as well as to the works which may be submitted to us for review. Although we have not adopted the harsh motto of a celebrated modern critic, still arrogance shall not be suffered by us to escape unreprehended, nor error of any kind to pass without notice.

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ART. I.—*Surgical Essays.* By Baron D. J. LARREY, Surgeon in Chief of the Hospital of the Royal Guard, First Surgeon of the Grand Army in Russia, Saxony, &c. &c. Translated from the French, by JOHN REVERE, M. D. Baltimore, 1823. Octavo, pp. 336.

NAPOLEON had so wonderfully excited the attention of mankind, that every circumstance connected with his death, as well as those which are recorded respecting his early life, is now viewed with an interest as deep as it is extended. It is not surprising, therefore, that the surgeon whom he especially honoured in all his great campaigns, has acquired much celebrity; or that the same person, whom, in his last will and testament, he pronounced "the most honest man alive," should come before the public with a strong claim to its confidence.

Long before the publication of the present work, Baron Larrey had been extensively known from the circulation of his "Military Campaigns." Although the marvellous cases, with which they abounded, were calculated to excite some degree of suspicion, nevertheless, the fearless manner in which all his statements were detailed, and the bold appeals which he constantly made to the testimony of his colleagues, enforced a general conviction in favour of his authority.

The remarkable attention which his numerous contributions to the *Dict. des Sciences Medicales*, and other periodical works, have invariably received, also attests the high degree of confidence with which his remarks are estimated by his brethren. Indeed no man can now be considered as occupying higher ground in the profession than this veteran practitioner. His opinions must, therefore, be regarded with deference, while his experience cannot fail to be esteemed as authoritative.

But, in attending to the claims of this so much celebrated author, we must not overlook the merits of his translator. To Dr. Revere, our medical brethren are greatly indebted for the pains he has taken to present them with a correct and elegant version of the "Surgical Essays." The original work is certainly one of the most important productions of modern surgery, and the translation is perspicuous and accurate, at the same time that it strictly preserves the spirit of the author.

But we have said enough concerning the general character of this publication, as well as of the degree of credit which should be attached to its details. We will proceed, therefore, to analyze the subjects of which it treats in the order in which they occur—not doubting that they will prove sufficiently instructive to repay whatever attention may be bestowed upon them.

The volume is composed of six different essays. The first of these treats of *the use of Moxa* in various functional and organic diseases; the second of *the seat and effects of nostalgia;* the third of *the properties of the Iris;* the fourth of *wounds of the intestines;* the fifth of *fractures of the neck of the femur;* and the sixth *on wounds of the bladder, and upon certain foreign bodies remaining in that viscus.*

The essay upon the use of Moxa contains the most practical and novel information. In this country that "heroic remedy" has, as yet, hardly been known to the profession. The slender accounts which were formerly received from those who had visited the Parisian Hospitals, were of so

discouraging and frightful a nature, that no one could summon resolution enough to give the remedy a trial. A late teacher in this city, used to relate to his class, that he had seen the celebrated Dubois sear the cheeks of a venerable old man until the fat ran down in streams to his very knees. Whether any representation of this kind influenced the practitioners of other countries or not, it is impossible to determine; but it is certain, that the use of the Moxa was chiefly confined, until a very recent period, to the hands of a few French Surgeons. It is now almost unnecessary to state, that the painful effects of the remedy have been greatly exaggerated by such reports; and that while our physicians continue to be prejudiced by them, they will be obnoxious to the charge of refusing to their patients one of the most efficient means of combating disease.

The term *Moxa*, is said to have been derived from the Japanese name of a lanuginous plant, the down of which is collected and rolled into small cones for the purpose of being applied, in a state of combustion, to the surface of the body. In India, a similar production was formerly gathered from several different plants; and spider's-webs have also been used for the same purpose. No peculiar virtue, however, can be ascribed to either of the articles thus employed. It is probable that any inflammable substance will prove as useful, provided it be light, and capable of exciting much flame. Baron Larrey has been in the habit of employing carded cotton, rolled up into cylinders, and sewed in small pieces of fine linen. These are applied to the skin by means of a small instrument, called the *porte moxa*, which is merely a metallic ring, supported on three little ebony balls, and attached to a handle. The balls, being nonconductors of caloric, and separating the metal a few lines from the skin, prevent the heated instrument from exciting unnecessary pain, while the combustion operates immediately on the subjacent part.

Before using the moxa, the part where the application is to be made is marked with ink. A wet cloth is then placed over the surrounding parts, to protect them from the flame.

"After applying the fire to the apex of the moxa, it is then to be fixed on the point marked out, and by means of the *porte moxa* retained in its situation, while with the blow pipe the combustion is kept up until the whole be consumed. To prevent deep inflammation and profuse suppuration, which would be the result, it is necessary immediately afterwards to apply on the point the volatile alkali."

The introduction of the blow-pipe Baron Larrey claims for himself, as a most important addition to the former method of performing the operation. Without it, indeed, the moxa produces comparatively but superficial and insignificant effects.

The employment of volatile alkali, also, to moderate the severe effects of the remedy, appears to be original with our author. As the oil of turpentine has been found so useful a remedy in common burns, we can have no difficulty in conceiving how the ammonia may prove beneficial after the application of a moxa. The stimulating powers of this article prevent sloughing and suppuration, by re-exciting the exhausted capillaries, in the same manner as we remove indirect debility of the general system, by various modes of excitation.

The employment of ammonia, after applying the moxa, is also attended with another important advantage, which is incidentally mentioned in several parts of the book. It immediately removes pain. To such a degree is this the case, that a repetition of the operation in a great measure ceases to become formidable to the patient. This corresponds with our own experience in the use of the remedy. We have not found, however, that ammonia, in every case, prevents suppuration; although in a majority of instances it has caused the inflammation to terminate in the formation of a scab. It is necessary, in order to produce these effects, that the volatile alkali should be strong, and in the form of pure *aq. ammoniæ* of the shops.

From these circumstances it has occurred to us, that ammonia would form an excellent application to common burns. We have tried it in three different cases, and its effects were so favourable as to induce us to believe that it will eventually

supersede the turpentine and other stimulating remedies, which are usually resorted to on such occasions.

The method which we have adopted in applying the moxa, is much more simple than that which Larrey details. With a pair of scissors a small hole, from half an inch to an inch in diameter, is cut out from the centre of a piece of wet cloth, which is placed on the surface of the selected part; a small roll of finely carded cotton, in a state of full combustion, is then held by a pair of dressing forceps immediately over the exposed spot of skin. The flame is directed upon the surface by gently blowing on it through a glass tube, or the barrel of a quill. It sometimes occurs that the cotton is with difficulty maintained in a state of combustion. By first reducing it partly to the state of tinder, this difficulty can be obviated. We have, also, mixed a small quantity of finely powdered nitre with the cotton, for the purpose of exciting the combustion. But this is attended with too great a flame, and, consequently, increases the pain of the operation. It is not absolutely necessary to envelope the cotton in linen cloth, as Larrey directs, although the heat will be rendered more intense by such a contrivance.

Larrey interdicts the use of the moxa on all that part of the cranium which is thinly covered by the integuments; also, on the eye-lids, nose, and ears; over the course of the larynx, trachea, sternum, mammæ, linea alba, organs of generation, superficial tendons, and capsular ligaments of the joints. But we cannot see any reason for absolutely condemning it in all these places. Provided it be only moderately applied, so as not to excite suppuration, there can be no objection to its use in almost every one of the localities here pointed out.

Some practitioners have been in the habit of using the actual, or metallic cautery. But its effects, according to the Baron, are vastly inferior to those of the moxa. They are more local, being almost limited to the point immediately touched by the instrument. The actual cautery also disorganizes the part to which it is applied; is accompanied by an intolerable pain, and is sometimes followed by a destruction

of the subcutaneous nerves, and a very copious suppuration. The moxa, which burns slowly, is less painful and alarming to the patient. Instead of exhausting the system by subsequent discharges, it has an invigorating effect by directly exciting the nervous powers.

But caloric is not the only agent which is supposed by our author to be derived from the moxa. To electricity is ascribed an important share of the influence which it exerts upon the body. "This remedy has also appeared to me, to communicate to the surrounding parts, together with a relative mass of caloric, *a very active and volatile principle*, furnished by the cotton-like substances during combustion. The excitation and irritation which result from the *two* products, (caloric and electricity) increased by blowing, propagate themselves by degrees to the deep seated parts, so as to re-establish the action of the nerves, which have become weakened or paralysed, and to arrest the progress of the morbid cause established in certain parts. When we wish to obtain superficial effects from the moxa, we have only to suffer it to burn, without using the blow-pipe." (p. 23.)

From these observations it appears evident that the Baron would explain the action of the remedy, on the common principle of counter-irritation. Indeed, he repeatedly calls the moxa an "*excitant and revulsive.*" When applied over the course of the nerves, however, it operates, differently from any irritant hitherto employed, by extending a peculiar excitement through all their ramifications, and imparting a new action to the solids to which they are distributed. These effects may be considered as somewhat analagous to those which are produced by passing the electric fluid through the nerves.

"To the exciting property of the moxa, which is most efficacious, may be added, if we wish, the revulsive and derivative property produced by the suppuration of the cauterized part, when we suffer this to take place. It is easy to distinguish the cases where it is necessary, from those in which it is useless, or even injurious." Suppuration will no doubt prove useful, in cases of an inflammatory tendency, on the

same principle that we derive advantage from setons and issues, but it will be apt to do harm when it is allowed to take place in exhausted constitutions.

The number of moxas to be applied for the relief of a chronic disease, is smaller than would, perhaps, have been anticipated. "We may apply one or two at a time; but it is necessary to leave an interval of several days between each application." A greater number at any one period, or a too frequent repetition of them, would, he thinks, be apt to produce traumatic fever and exhaustion.

"A humid state of the atmosphere is not so favourable to the success of the remedy, as clear and dry weather." This opinion, which we believe to be a correct one, must probably have been formed from a consideration of the electrical properties of the remedy. Damp weather is always unfavourable to the developement of electricity.

To promote the efficacious effects of this remedy, he recommends, in many cases, to precede its application either by dry or wet cupping, and to follow it by the internal exhibition of suitable remedies.

Cupping, he thinks, is always more effectual when performed by burning cotton, or some other inflammable substance, within the vessel. This is attended, in some respects, with the beneficial effects of the moxa, of which the exhaustion of the air by a syringe, is altogether destitute.

The baron has employed his favourite remedy in a large number of local diseases, several of which have been considered as incurable under the ordinary modes of treatment. The success which he derived from it is really surprising, and would hardly be credited on the authority of an inferior name. Our readers, however, must bear in mind that proper auxiliaries should always be resorted to in the administration of this, as well as of most other remedies. Vascular excitement should be previously reduced by means of general or local abstraction of blood. Occasionally a frequent repetition of blisters, and an issue, or seton, are necessary to relieve engorgement of the vessels in the affected part. Purgatives,

and, perhaps, emetics, will also be sometimes required to remove visceral derangements and nervous irritation. These circumstances having been premised, the application of moxas, according to our author, will rarely fail to relieve such local diseases as are not attended with a complete alteration of structure. Even in cases of actual disorganization it has afforded the greatest benefits, as will be seen when we come to detail the history of its success in diseases of the bones and joints, and in pulmonary consumption.

In various affections of *vision*, the practice of our author appears to have been successful. "The imperfect action of the membranes composing the globe of the eye, incipient cataract, and recent weakness, or paralysis of the optic nerve, truly indicate the application of moxa. It should be made in the course of the nerves most intimately connected with those of the eye, such as the trunk and principal branches of the facial, those of the superior maxillary and of the frontal. "By this remedy I have particularly stopped the progress of *amaurosis*, and have caused it to disappear, in some instances where the blindness was complete."

It does not appear probable, however, that this remedy will ever be of much service in any stage of real cataract; although such obfuscations of the transparent media, as depend upon a congestion in the deep seated veins, may be relieved by it. In the other affections which he has mentioned, a greater prospect of success may be anticipated.

We have now employed the moxa in three different cases of diseased eyes, with very evident advantage. The first case was that of a mulatto, in November last, who was afflicted with chronic ophthalmia, attended with thickening of the lids and pustules in the conjunctiva. A few laxatives, with a moxa four times repeated over the orbitar nerves of each eye, effected a speedy cure. The second case occurred in the person of a lady from Lancaster county, who had spent some months in town, under the care of several physicians, for the relief of a very afflicting morbid sensibility of both eyes. She had undergone ptyalism, setons, vesications,

the use of emetics, purgatives, &c., without the least perceptible advantage, and began to despair of relief. Applications of the moxa, however, over the trunks of the facial nerve behind the jaw, and the super-obitar on the forehead of both sides, repeated at three different periods, with intervals of four days each, gave her almost complete relief. So great was her confidence in the remedy, that she bore every repetition of it without a murmur, and regretted being obliged to leave the city without a longer trial of its virtues.

The third case is now under treatment. It is one of *complete amaurosis*, which supervened, about eighteen months ago, on an attack of bilious fever. The patient is a thick necked plethoric man, twenty-eight years of age, from Mifflin county, in this state. He came to the city twelve months ago, in a state of almost total blindness, and at first experienced considerable relief from the use of cuppings, issues, and emetics. Two paroxysms of epileptic convulsions, however, which occurred in June and August, from errors in diet, destroyed all the powers of vision that he then possessed. Subsequently, he was put upon the daily use of active cathartics, was frequently blistered, and had issues discharging from his lower extremities; but all to no purpose. The disease became more and more confirmed, and he finally went home in despair, some time in December last. But he was induced to return to the city about three weeks ago, in consequence of an idea, which his friends entertained, respecting a change in the nature of his disease. The pupils, which before were greatly dilated, had now become contracted to their natural size; and his general health was also amended. No absorption of the pigmentum nigrum, or discolouration of any part of the ball, had taken place. Still he remained perfectly blind; not being able to distinguish the least impression of light, except in the sunshine, when it appeared probable that caloric was most concerned in exciting the sensation which he experienced. Believing that the retinas were more or less paralysed from previous congestion in the choroid vessels, as well as from long continued disuse, we conceived

that he would present a favourable case for a trial of the moxa. Accordingly, on the third day after his arrival, moxas were applied over both facial and super-orbitar nerves. The result of these applications was that, in two days after, he could distinguish with his right eye the situation of a candle at night, and of a window in the day time. The right pupil also became moveable to the light. The moxas have been repeated three times since, with intervals of four or five days each, as the weather would permit; and his vision has been regularly increasing. After the second application, he could distinguish the direction of a hat carried between him and the light; after the third, his left pupil began to move, at the same time that the corresponding nerve became sensible; now he can discern the figure of a man standing before him with both eyes. We are encouraged, therefore, to hope for complete success in this case.

In affections of the sense of *smell* our author has obtained no success from the application of moxa. "It appears that the particular modifications that the olfactory nerves undergo in the pituitary membrane for receiving the impression of odours, renders the nervous tissue of this membrane inaccessible to the exciting and electric effects of moxa." He has also learnt from experience that moxa has no effect upon the sense of *taste.*

In *deafness,* however, he has been more successful. "When deafness is the result of the impression of a sedative, stupifying cause—as cold, applied suddenly to the ear, or the influence of a current of humid air directed upon this part, the moxa, if applied over the branches of the facial nerve, and about the auditory opening, restores the function of hearing. The calorific excitation of this cautery communicates the more readily with the auditory nerve, as it has intimate anastomoses with the small sympathetic (facial); this remedy cannot be supplied by a vesicatory. I might relate a great number of cures of deafness, from the employment of moxa, &c." He relates an interesting case of a young trumpeter, who, from bathing while in a full perspiration, was suddenly de-

prived of his *voice*, and the power of *hearing even the loudest sounds.* Thirteen moxas, preceded by cuppings, completely restored him to the use of his voice and hearing. We may add from our own experience, some testimony in favour of the remedy in this complaint. We have now employed the moxa in three cases of deafness, apparently unattended with any organic defect. In one of these a single trial of it has been attended with evident advantage. In the other two, repeated applications of the remedy have afforded considerable relief, and we are encouraged to continue the treatment.

In most cases of *spasmodic affections of the muscles*, the moxa is indicated, "but it should be applied as near as possible to the seat of the disease, and upon the course of the injured nerves." "It is not indicated in *acute neuralgia*, nor in *tetanic* affections, because it augments the irritation and tetanus."

In *chronic neuralgia*, (tic doloreux) a disease considered by almost all physicians as incurable, the moxa has proved eminently successful. He relates three cases in which complete relief was obtained by a few repetitions of the remedy. Its effects do not appear to be influenced by the cause of the disease, provided that proper auxiliaries be previously resorted to, for the purpose of removing arterial excitement. One of the cases, which he has detailed, originated from a blow on the malar bone; the other two could be traced to no external cause whatever.

The pathology which he advocates is very common among the French writers, although it can by no means be considered as unexceptionable. "The lesion consists in the chronic engorgement and inflammation of the membrane which invests the nerves of the part affected. This remedy, by producing an excitation upon these organs, causes a salutary derivation of the morbid principle, and re-establishes the course of the nervous fluid." In drawing up this hypothesis, which, we do not deny, is applicable to one variety of the disease, the baron must have overlooked the cases which depend on a primary disorder of the digestive apparatus; as well as those

which originate in some derangement of the cerebral structure. Whether or not this remedy will prove equally efficacious in the last mentioned varieties, we are not prepared to determine; but we are convinced that the preliminary treatment will be most essential to a cure. In those forms of tic doloreux, however, which are wholly confined to the painful part, there can be no doubt of the immediate powers of the moxa. We have lately employed it with remarkable success in the case of a young man, who had lost both his eyes by receiving a discharge of bird-shot in them, sometime in July last. But the loss of vision was not the most distressing circumstance in his case. Many small shot were lodged underneath the integuments of his forehead, which, by irritating the branches of the super-orbitar nerves, excited those convulsive and intolerable pains that characterize the *tic doloreux*. The pain came on in paroxysms with irregular intervals; and no mitigation of it was experienced from any mode of treatment until the month of February, when, by a few moxas applied on two different occasions, a complete cure was effected.

In the case of a little boy, however, who is still under treatment for severe neuralgic pains of the head and face, the moxa entirely failed, although it was fully applied and often repeated. But it should be observed, in explanation, that, the pains are probably excited by a large portion of the upper maxillary bone which is in a state of necrosis from scrofula.

The baron, also, speaks highly of the moxa in *paralysis*. He divides this disease into several varieties. The first is limited to an atony of the locomotive powers, without any loss of animal sensibility; in the second, which is very rare, the contraction of the muscles remains perfect, while the animal sensibility is entirely lost; in the third these two properties are affected at the same time, which constitutes complete paralysis. He has often found, also, that paralytic affections of the muscles are accompanied with an increase in the animal sensibility; and these are characterized by pain, with unnatural and involuntary motions in the affected member.

In the last affection, which has some affinity with *tic dolo-*

reux, the moxa should be allowed to suppurate, and it should always be preceded by cupping with scarifications. In the three first varieties, suppuration is not necessary; indeed, it would probably be injurious.

He does not approve of the use of the *nux vomica* in palsy; its effects are constantly pernicious, and he thinks it should be proscribed from practice. In all those cases which are attended with irritation and thickening of the nerves, there can be no doubt that this poison will tend to aggravate the neuralgia, without increasing at all the tonic power of the muscles.

He relates three remarkable cases, in which the moxa proved successful. 1*st*. Mr. P. an advocate of Paris, had been afflicted for three years by a gradual wasting of his strength, from a paraplegia, with neuralgia, marked by an inability of standing or walking, violent pains, nearly constant, trembling of the lower extremities, insomnia, and extreme irascibility. All the usual remedies in such cases had been tried in vain. After having applied several series of cuppings over the lumbar region, and in the course of the principal nerves of the lower extremities, he began the application of moxa, two at a time, commencing from that part of the vertebral column where the disease appeared to begin. This was about the tenth or eleventh dorsal vertebra, the spinous processes of which made an unnatural projection at that point, and the part was very painful to the touch. The first applications calmed the pains, and encouraged the patient. The two next were immediately followed by spontaneous motions in the diseased limbs, and a delightful calm, that, for the first time for a great while, permitted the patient to sleep sound. After the eighth moxa, he could stand upright and walk a few steps by means of crutches. The pains and tremors had entirely disappeared by the tenth application, and the contractibility of the muscles was sensibly increased. After this, every application evidently augmented the force and action of all the vital properties in the limbs, so that their nutrition was equally re-established. After the thirty-second application, the cure had become as perfect as possible. The patient could walk for a long time

by the assistance of a cane, without pain or inconvenience. The pains in the extremities were no longer felt, and the limbs recovered by degrees their primitive form and size.

This paraplegia presented a remarkable phenomenon, which he had never seen so strikingly displayed in any other case. "Every application of the moxa produced contractions as strong in the feet and legs as those which result from galvanism directed upon the denuded nerves of recently amputated limbs." A very plausible argument in favour of the electrical properties of the remedy might be derived from this fact.

The other two cases occurred in the persons of two soldiers of the imperial guard, affected by paralysis, with neuralgia of the fore arm and hand, in consequence of gun-shot wounds of the arm. Both of these were perfectly cured by the moxa, after all other remedies had failed.

Hemiplegia of the face has also been treated by the moxa with great success. Young soldiers were frequently attacked with this disease, in consequence of sleeping upon the wet ground in the open air. The eye of the affected side remained open during sleep, the angle of the mouth of the opposite side was retracted, in consequence of the action of the sound muscles. "The repeated application of small moxas over the course of the branches of the facial nerve, and some of the anterior branches of the cervical pairs, restored in these patients the paralysed muscles." In one case of much longer duration it also proved successful. A young lady, seventeen years of age, had been afflicted from her infancy with a hemiplegia of the left side of the face, supervening on a verminous fever. The deformity was extreme, especially upon the least smile. The moxa was applied in small cones along the course of the trunk and branches of the facial nerve, and followed by the use of ammonia, so as not to excite suppuration. At the fourth application, there was a sensible change in the disease; the improvement, however, was but slow, until the ninth application. After the seventeenth application, the cure became perfect.

We have employed the moxa in one case of the same dis-

ease. A Spanish Creole, from the island of Cuba, was afflicted with a partial hemiplegia of the right side of his face and head, and a chronic inflammation in his eyes, which resulted from rheumatism. He exhibited a proptosis of the right palpebra, with an evident paralysis of the zygomatici and levatores muscles of the same side. Three applications of the moxa, preceded by cuppings and a seton in the nape, completely removed the hemiplegia, at the same time that they, in a great measure, relieved the eyes.

It is more difficult, however, according to our author, to relieve *hemiplegia of the extremities.* He attributes this circumstance to the remoteness of the affected members from the brain and spinal marrow. On a similar principle, and with more plausibility, he asserts that fat people receive less benefit from the remedy in proportion to their bulk. When the paralysis is recent, and the subjects are thin, we may obtain a complete cure. He has treated a great number of soldiers affected with hemiplegia, from the excessive cold which they suffered in the Russian campaign. The moxa, applied on the sides of the vertebral column, and upon the course of the principal nerves of the limbs, produced astonishing effects. The cures, however, proceeded slowly.

Paralyses of the muscles of the superior extremities, which result from wounds, also yield to repeated applications of the moxa. It should be applied above the cicatrices, and over the course of the injured nerves. He relates an interesting case of a young soldier, in whom an entire loss of animal sensibility in the right superior extremity, resulted from the stab of a sabre, above the clavicle. Three moxas, preceded by cupping with scarification, were sufficient to restore the sensibility of the whole limb, and place it in the same situation as the rest of the body.

It was remarkable, that although the affected member exhibited no kind of sensibility under any stimulus, still its motions remained perfect, and were never suspended for a single moment. Larrey attempts to explain this fact, by supposing that the sabre only touched a few branches of the

cervicals, which are destined to form the cutaneous nerves. But a similar phenomenon is presented in cases of injury of the spinal cord. Sometimes the muscular powers are altogether paralyzed, while in other instances the animal sensibility will be exclusively affected. In such cases no explanation of the kind above advanced can be considered as admissible.

An instance of complete paralysis of one arm from injury of the nerves is now under treatment in the Philadelphia Alms House. It occurred several months ago from a dislocation of the humerus into the axilla, which probably contused, if it did not lacerate, the plexus of nerves in that region. All the modes of treatment usually adopted on such occasions had been tried in vain, before the use of moxa was commenced. Although that remedy has now been applied but a very few times, the condition of the limb is rapidly improving, both as it regards sensibility and motion; and a complete cure is anticipated by the attending physician.

According to our author, a simple muscular paralysis of the inferior extremities, resulting from an injury of the spinal marrow, may be relieved by the moxa. The viscount Gen. M——, was seized with a paralysis of the inferior extremities, with an unnatural projection of the spinous processes of the lower dorsal vertebræ, deep seated pains in this region, and retention of urine. All the organic and sensitive functions were impaired. Nine applications of the moxa, with purgatives, a catheter in his bladder, &c., effected a perfect cure.

In all *chronic diseases of the head*, he has employed this remedy with great success. In idiopathic epilepsy, hydrocephalus, chronic cephalalgia, &c., &c., the application should be made on the base of the cranium, and especially about the union of the squamous sutures, with the lambdoidal. He relates five remarkable cases of diseases in the head, which were treated by him.

First.—A young trumpeter, in consequence of a fall from his horse, suffered epileptic fits for two years, frequently twice in a day. His cranium was much disfigured, and had

acquired, in a short time, a considerable increase of size. The eyes were very prominent, and almost immovable, the pulse slow, the respiration laborious, and the extremities almost always cold; standing and walking were performed with difficulty, and all the sensitive functions and the mental faculties were very imperfect. After a free bleeding from the jugular, repeated cupping, ice upon the head, sinapisms to the legs, and the internal exhibition of calomel, fifteen moxas were applied about the head, till, by the end of the tenth month, the patient found himself perfectly cured.

Second.—A grenadier had all the symptoms of hydrocephalus internus, and was cured in four months by similar treatment.

Third.—He obtained the same success by the same means, in Mrs. J——, an English lady, in whom an acute dropsy of the right ventricle occurred suddenly, so as to produce apoplexy and hemiplegia of the left side. The late celebrated John Bell assisted him in the treatment of this case.

Fourth.—A child, eight years old, exhibited all the symptoms of a chronic dropsy of the ventricles, with enlargement of the cranium. After the usual treatment, he applied upon the nape and on the sides of the head several moxas, and afterwards the actual cautery twice over the posterior and lateral fontanelles. In nine months this little patient was restored to perfect health.

Fifth.—Another child, nine years of age, of a scrofulous habit, and affected by the same disease, characterised by the same symptoms, was cured by the same means.

He has also removed entirely by the moxa, applied on the sides of the head and occiput, chronic and rheumatic headaches, which had resisted a great number of remedies.

In mental diseases, attended with excitement, the moxa is *contra-indicated*, however highly it may be extolled by some authors.

In several diseases of the chest, our author reports favourably of the moxa. He has employed it with great success against *asthma*, when it was not hereditary, or produced by

a bad conformation of the thorax, and the subject not too far advanced in life. He placed cylinders of moxa in two parallel lines on the sides of the chest, towards the anterior attachments of the great pectoral and serrated muscles.

Intermittent, nervous palpitations of the heart, arising from weakness of this organ, and of the spinal marrow, are successfully combatted by moxa, if applied on the sides of the vertebral column, and about the region occupied by the heart.

The remedy is also efficacious in *old catarrhal affections, and chronic inflammations of the pleura.*

In *consumption,* however, the highest encomiums are lavished upon the moxa. It produces "a resolution of the lymphatic engorgements, or scrofulous turbercles, and of symptomatic abscesses, or abscesses by congestion, when they are not too far developed. It deterges internal ulcers, stops caries of the bones, produces adhesion and cicatrization of the walls of abscesses, and purulent cavities established in the substance of the lungs, or any other part become the seat of phthisis. In a word, the patient is conducted to a complete cure, &c."—"The internal remedies, more or less extolled by authors, as the acetate of lead, and particularly the prussic acid, are for the most part injurious, at least useless. If, however, there be any reason to suspect a particular virus, it will be first necessary to destroy this cause, and when the disease shall have become insulated, to attack it by the moxa." "We should select, in its application, those parts of the chest which are nearest to the diseased portion of the lungs. There can be no doubt that the hollow cylinder of wood used by Laennec, would be favourable to this research; but the experienced physician has no need of it. Percussion, pressure made with care and habit, and the impression exerted by the fingers between the ribs, will enable us to assign, with great certainty, the seat of the disease."

Seven cases, which presented all the symptoms of genuine phthisis, and which were either partially or fully cured by the use of moxas, aided by other appropriate treatment, are related by the Baron with great minuteness. The celebrated Laen-

nec attended with him on several of these cases, and ascertained, by the stethescope, not only the existence of ulcerated cavities in the chest, but their subsequent progress, also, towards a complete cure.

The moxa has also been known to produce a resolution of hydro-thorax and hydrops pericardii, while in an incipient state.

In several chronic and organic diseases of the *abdominal vicera*, the moxa is recommended by our author. 1*st*. Of the *stomach*. The engorgements of the coats of this viscus, and the schirrhus, which is ordinarily the result of them, even after the passage of the aliments has become obstructed, may be relieved by the frequent application of the remedy upon the epigastrium. 2*d*. Obstructions of the liver, spleen, or any other abdominal viscus, may be attacked with equal success by the moxa, especially when the disease has not arrived at its greatest degree of development. 3*d*. We may produce a salutary revulsion in chronic engorgements of the *uterus*, which are almost always followed by cancerous ulcerations, by the application of moxas over the lumbar regions, preceded by cupping and laxatives. He has removed this disease by these means in several women, who had strong symptoms of it.

But the moxa has been attended with the most signal advantages in the different forms of *osseous phthisis*, or "that asthenic, rheumatic, or scrofulous affection which generally attacks young persons in the fibrous cartilaginous and bony structures, such as the spine, the junctions of the bones of the pelvis, and the articulations of the extremities." He divides this disease into two kinds, *Rachitis* and *Rachialgia*. The great difference between these two consists in this, that the former is unattended with caries and ulceration, which are the usual consequences of the latter. The former originates from a defective assimilation of food in infancy and childhood; while the latter is frequently found to depend upon a rheumatic disposition, as well as upon a scrofulous habit, and does not usually occur so early in life.

"The effects of *rachitis* are softening of the bones, curvature of the vertebral column, and more or less gibbossity. The moxa is, without question, the best remedy against this disease." Pouteau recommended it highly; but Desault was the first who ascertained that the success of the remedy is most certain in this disease, when the wounds, or eschars made by the moxa, are not suffered to suppurate. The application should be made as nearly as possible over the course of the posterior branches of the vertebral nerves, between the transverse processes, so as to communicate at the same time with the spinal marrow. The Baron does not approve of corsets, or other mechanical means. Indeed he thinks them, in general, highly pernicious. "They will serve, to a certain extent, in pressing down the curvatures or projections of bone; but if the morbid process continue, the disease will develop itself towards the opposite points, so as to affect the integrity of the internal organs. It is necessary, therefore, to proscribe the use of these machines, and limit ourselves to the moxa, and internal remedies, which will have a tendency to second its effects. I could relate a great number of cases of the successful application of moxa in this disease."

Rachialgia is considered by our author as a much more formidable disease. He thinks it to be "in all cases, either a rheumatic or scrofulous affection established in one of the points of the vertebral column, the principal effect of which is a latent or chronic inflammation in the fibro-cartilaginous and bony tissues of the vertebræ; it is a true phthisis. This inflammation, so far from increasing the volume of the parts by engorgement, weakens their tissue, and appears to accelerate the process of absorption and decomposition, so that the bodies of the vertebræ, where the alteration first takes place, by degrees become softened. The intervertebral cartilages are first decomposed or dissolved; to this effect, absorption, erosion, or caries of the osseous substance, in corresponding points soon succeeds, and develops itself, more or less rapidly, according to the intensity of the causes, the age, and idiosyn-

crasy of the individual; the caries rarely attacks the spinous, or transverse processes. At the first moment of the erosion, there is emitted by all the diseased vessels a purulent, serous fluid, which accumulates at first under the membranes, or investing ligamentous texture. Afterwards it becomes extravasated through the cellular structure towards the most depending points, or towards those where it meets with least resistance; and accumulates at places more or less remote, where it produces a symptomatic abscess, or abscess by congestion."

The symptoms which first indicate the existence of rachialgia, are obscure, deep-seated, local pains. They at last become increased, and are propagated in the course of the spinal marrow, and nerves which emanate from it; especially those which go to the extremities nearest to the disease. The action of the muscles of these parts become impaired, without being paralysed; spasmodic pains, and a sort of tension, or accidental retraction, with a sensation of cold, independent of the air, are experienced in those limbs. To these symptoms are added dullness, oppression, loss of appetite, emaciation, slow fever, with irregular intermissions, followed by colliquative sweats, and marasmus. It has been designated by the names "vertebral disease," "curvature of the spine," or "*maladie du Pott.*"

The baron does not approve of the vegetable caustic recommended by Pott. The copious suppuration which it occasions weakens the patient very considerably, without producing the desired revulsion, especially if there be symptomatic abscesses. If these abscesses are opened early, before efficacious means have been employed against the caries, whatever may be the process, the patient dies very soon. Now, in the administration of these means, it is necessary to endeavour to stop the process of suppuration; it is sufficient to produce an excitement on the parts affected, to divert the morbific principle, and to change the vital properties of the inflamed parts. Moxa, preceded by cupping, with scarification, fulfils this double indication. A great number of patients, whose situa-

tions were considered desperate, have owed their safety to this heroic remedy.

Nine cases are related, in which complete cures were effected. One of these, "General L——, exhausted by a variety of debilitating causes, was attacked by a dorsal consumption, slow fever, great loss of power in the genital organs, curvature of the spine, uneasiness and numbness in the lower extremities, and marasmus in the first degree. This affection had resisted a great number of remedies. It was decided in consultation, that a series of moxas should be applied upon the vertebral column and the region of the sacrum, without discontinuing the bark and ferruginous preparations at that time administered. With the three first applications a favourable change took place; the strength of the patient returned in proportion as the applications were repeated. At the seventh, he was in a state to walk alone; and by the thirteenth he went to the mineral waters to complete a cure already far advanced. He has made several campaigns since."

"Mademoiselle D., twenty-five years of age, was in the first stage of marasmus, with strong symptoms of pulmonary consumption. The dorsal vertebræ were curved backwards and towards the right side. The scapula of the same side was detached from the trunk nearly an inch by a soft tumour, or incipient abscess by congestion. These symptoms announced the first stage of a caries of the vertebræ. The debilitating regimen, to which she had been subjected for several months, was replaced by one that was tonic and nourishing; the bark, combined with opium, and balsamic substances were prescribed. Twenty moxas were applied successively, after intervals of three or four days, upon the sides of the spinous processes of the dorsal vertebræ. The first favourable change was the sudden and entire cessation of the symptoms of pulmonary consumption; and soon after a reduction of the projection of the dorsal vertebræ, the resolution of the subscapular tumour, and the return of the scapula to its natural position. By degrees the general powers of the system returned, and

the internal organs recovered their functions; in a word, this young lady now enjoys perfect health."

The other cases were of a still more alarming character; but it is unnecessary to detail them on the present occasion.

As we have already observed, our author altogether disapproves of the practice of opening the abscesses in the ordinary way. After having used the moxa, however, until the progress of the disease was arrested, he has opened the abscess, in some cases, obliquely by means of a narrow knife, heated to incandescence. All the purulent matter should be instantly evacuated by dry-cupping, and a slightly compressive bandage applied.

Another process is recommended by him as still more advantageous, when the fluid contained in the abscess has spread into a portion of the cellular tissue, which communicates with the purulent sack. This consists in *passing a seton through the cellular tissue*, exterior to the sack: the fluid will pass out at the same time by both the wounds, and will continue gradually to ooze out until it is entirely evacuated. Then, if the caries of the bone which had furnished this fluid be stopped, as we must suppose in this case, the complete cure of the patient will be the more certain, as the matter of the abscess will be evacuated gradually, and without allowing the external air to communicate with the purulent cavity. Two cases, which were treated advantageously by this method, are related.

We have now employed the moxa in three cases of rachialgia, with evident success. In the first of these, an interesting girl, sixteen years old, a curvature of the dorsal vertebræ had been progressing for three years towards the right side, until a great deformity of the chest and shoulders was produced. Steel had been administered for a long time; and the machine, first described by M. le Vacher, resorted to without affording the least advantage. She became extremely weak and emaciated, and, in the month of March last, was seized with severe pleuritic pains in the left side of the chest and region of the curvature. Her pulse was at the same time very much

excited; her skin was hot, and respiration laborious. By several venesections, and repeated cuppings with scarification, these symptoms were subdued. There still remained, however, for some time afterwards, a sensation of great weakness and oppression in the back part of the chest and spine; and genuine hectical exacerbations, followed by exhausting sweats, were repeated every afternoon. For these symptoms digitalis, alterative doses of blue-pill, antimonials and laxatives, were perseveringly employed, until the excitement was reduced. The moxa was then resorted to, and repeatedly applied, in the way already described, over the region of the curvature. The first applications were followed by very sensible relief; the second so far removed the weakness of the back and extremities, as to allow her to stand erect and walk across her chamber with considerable ease. After the fifth, she was altogether freed from every disagreeable sensation in the affected parts, and could remain out of the chamber to which she had been confined, during the whole of the day and evening. By strictly adhering to a diet of milk and farinaceous articles; and by using alteratives and laxatives, with moderate exercise, she has continued to improve in her general health. There is now every reason to hope for a perfect recovery in her case, with the exception of the deformity, which has become permanent.

The second case occurred in the person of a sister of the former, two years older, and of a more vigorous constitution. She had a double lateral curvature, and complained of tenderness on pressure over the spinous processes of the distorted points, with an almost constant sensation of pain and weariness on the convex side of the lowermost curvature. She was put on an alterative course of medicine, with purgatives; and she was restricted to the same diet as has just been detailed. After one full cupping over the region of the curvature, the moxas were four times applied with the effect of completely relieving all her uneasiness and debility.

The third case is that of a young child, five years old, with a posterior projection of the upper lumbar vertebræ, and a

partial paralysis of the inferior extremities. Purgatives had for a long time been employed, and issues, such as were recommended by Pott, had been kept discharging during several weeks. Although these remedies improved the constitution of the child very much, and relieved the weakness of her extremities to a considerable extent, still she remained in a disabled state, tottering, and staggering in her walk, and falling down over the slightest obstruction. Two applications of the moxa, on each side of the curvature, have already removed the paralytic symptoms, and a few repetitions of the remedy will probably conduct her to a complete cure.

Sacro-coxalgia. A separation of the sacro-iliac suture may result from external violence, and from the delivery of uncommonly large children in young women. Such cases, several of which have been treated by our author, are usually managed by retentive and compressive bandages. A separation of the same suture, however, sometimes occurs from rheumatic inflammation of the bones, and constitutes a disease precisely similar to rachialgia in the spine. A case of this kind was cured by M. le Heritier; who, after having used the actual cautery, ingeniously contrived the idea of fixing the two bones together, by means of an elastic bandage. The frequent application of the moxa offers a powerful auxiliary to this plan of treatment.

The same kind of affection sometimes attacks also the sterum, (sternalgia) the ribs, (costalgia) and the scapulas (scapulalgia); and the results of this disease, established in the substance one or more of these bones, is altogether the same as in the preceding cases. The abscesses, which take place, as in rachialgia, more or less remotely from the focus of the disease, are invariably produced by the caries of these bones. He has also remarked, that when the opening of the abscesses takes place spontaneously, before the caries in the bone producing them has been stopped by the means already pointed out, the disease is always fatal. On the contrary, when the moxa is applied early, so as to stop the progress of the caries, the operation of opening them is followed by hap-

py consequences, of which the Baron has seen numerous instances.

Femoro Coxalgia, or the hip-joint disease, is, also, of the same nature, resulting from a latent and chronic inflammation of the apparatus of the coxo-femoral articulation. It is ordinarily the effect of a rheumatic affection, or exhaustion of the prolific powers of the patient. It may be, moreover, the effect of an hereditary or acquired scrofula. When it arises from the former causes, it rarely attacks very young, or very old persons, manifesting itself generally from the first period of puberty to the age of manhood. When it depends on the latter cause, it may attack the youngest children as well as adults.

He divides this disease into three stages. *First*,—elongation of the diseased limb, from the state of relaxation and paralysis, into which the muscles surrounding the joint have fallen; and to the engorgement, and inflammation of the synovial membrane and inter-articular ligament. The *second* stage is marked by a dislocation of the joint, from some slight external cause, after the erosion of the round ligament. He thinks the dislocation never takes place spontaneously. The *third* stage is characterised by the progress of the caries, the developement of the abscesses externally, and a febrile and cachectical state of the patient.

When the disease has not passed the first or second stage, it is susceptible of cure, especially if the patient be removed from the action of the causes which have produced it. But if it has arrived at the third stage, it is much more difficult to stop its progress and obtain a cure, although some examples of recovery are mentioned as having occurred to our author.

In the first stage, the inflammation of the articulating parts should be relieved by cupping with scarifications applied repeatedly around the joint. Afterwards, if the inflammatory symptoms remain, or if they return in the course of the disease, he recommends to introduce a seton into the fold of the thigh, through the integuments and cellular texture. The moxa, afterwards, produces the happiest effects.

Professor Rust, of the university of Berlin, used with great advantage, and without any preparation, the actual cautery, which he applied about the articulation in three oblique lines, uniting at the trochanter. He observed, immediately after the cauterization, that the limb was suddenly restored to its natural length, and became level with the opposite.

But our author has observed that, when the practitioner confines himself to the employment of the metallic cautery, the elongation of the limb gradually returns, and that those symptoms of the disease, which had for a short time ceased, soon return. He declares, however, that we may prevent their return by the repeated application of moxa, and cure the disease by persevering in its use. He does not think the metallic cautery absolutely necessary, although it concurs powerfully in assisting the action of the moxa; which, from its not acting with the same energy, does not stop so promptly the progress of the disease.

If caries has commenced, and there is a purulent collection, the effects of the moxa are less prompt and efficacious. As they often succeed, however, practitioners should feel encouraged to persevere in the use of them. "The violent, but gradual excitement, which the moxas communicate to the diseased parts, stops the morbid process, and appears to augment the action of the absorbents; so that the fluids, accumulated in the abscesses about the joint, or distant from it, if they be not too much distended, are absorbed and thrown into the circulation. I am ignorant of the parts through which the absorption of this matter is effected; but I am inclined to think, that it is through the cellular tissue and venous system. In all these cases it is announced by the diminution of the tumour, and a pustular eruption, which takes place over the whole surface of the individual, as well as by the earthy and purulent sediment of the urine, which is constantly precipitated, during rest, to the bottom of the vessel."

The Baron strenuously opposes the common plan of opening the abscesses, until, as in rachialgia, the caries which gives origin to them be stopped by the use of moxas and other ap-

propriate remedies. When it becomes necessary to open them, he recommends to perform the operation with a red-hot lancet; and immediately afterwards to evacuate their contents completely, by means of dry cupping over the orifice. A thick compress, soaked in camphorated oil of chamomile, should then be applied upon the external wall of the abscess and kept in its place by a compressive bandage. By these means the opposite parietes become agglutinated together, and contract mutual adhesions to each other, so as entirely to obliterate its cavity.

The most absolute rest should be enjoined upon our patients, for a long period after the use of these remedies. The Baron extends this term to the length of five or six months. At the same time, we should feel inclined to pursue the alterative treatment recommended under the head of rachialgia.

Our author relates seven very interesting cures of the hip disease, which we have not room to detail. We have not had any experience in this disease since the publication of the work under review; but we are strongly persuaded of the efficacy of the treatment which we have described.

He has, also, employed the moxa "with great success in lymphatic diseases of the other articulations of the limbs;" especially in that called white swelling of the knee, respecting which he intends shortly to publish another essay. Our own recent experience, in two cases of white swelling of the elbow joint, induces us to think very highly of the use of moxa in such cases; and we are disposed to allude to them as conclusive examples of the influence of our remedy in organic derangements. One of these occurred in the left arm of a negro child, from scrophula; the other took place in the right elbow of a coach trimmer, fifty years of age, from rheumatism in an intemperate constitution. The tumefaction in both these cases was enormous; the pains very severe; and fistulous ulcers, which existed, allowed a probe to pass into the very substance of carious bones, both above and below the joints. A cupping having been previously resorted to by the coach-trimmer, a number of moxas were applied on opposite points.

around the diseased part, in both cases. The applications have now been repeated four times in each case, with intervals of four or five days; and already the swellings have nearly subsided, the pains have been removed, and the fistulas have closed. The moxa is entirely depended on, and will, in all probability, effect complete cures.

We have extended our observations upon the first essay of the work before us to this length, because we conceive that the moxa is the most important, as well as novel subject, which can be introduced to the notice of our readers. We shall, on some future occasion, examine the interesting points of a pathological character, which have been presented to us by the remaining essays. Every thing which the Baron has discussed in the present work, is full of practical observation and interesting inquiry. If we have only succeeded in exhibiting one of his favourite topics in a distinct light, our readers will not remain satisfied without procuring the volume with which his translator has favoured them. M.

ART. II. *Recherches et observations sur les effets des preparations d'or, du Docteur Chrestien dans les traitements de plusieurs maladies et notament dans celui des maladies syphilitiques;* par J. G. NEIL, M. D. Paris, 1823. Octavo, pp. 391.

Researches concerning the medicinal effects of Gold, &c. &c.

During the superstitious ages of alchemy, gold was highly esteemed as a medicine. The *aurum vitæ,* as well as the *aurum potabile,* is spoken of by Salmon, an English writer of the 17th century, as a remedy of extraordinary powers.

"The former of these preparations," he says, "prevails wonderfully against dropsy, jaundice, plague, pleurisy, rheumatism, scurvy, leprosy, elephantiasis and *French pox;* for it purifies the blood, and strengthens the marrow in the bones, and radically drives forth all noxious matters or humours, out of the human body. It is a famous medicine, and deserves, by the greatest right, to be called a *panacæa:* it kills worms and heals all venereal ulcers;

purges both by vomit and stool, and taken in somewhat too large a dose, causes salivation, if often repeated."*

Extravagant as these praises are, gold did not long sustain its reputation as a medicinal agent; and, by the middle of the last century, it was scarcely known in the materia medica. Within the last twelve years, however, its powers have again been introduced to the notice of the profession, by M. Chrestien, in a work published in 1811, entitled, *Méthode Iatraleptique.* In 1814, a second edition of this work appeared, embracing much additional testimony in favour of the good effects of this remedy. Upon the authority of this writer, it was soon afterwards prescribed in various countries by men† of respectable standing in the profession; and their reports of its effects were, for the most part, quite favourable. In 1818, M. Chrestien drew up a full account, chiefly from his own experience, but partly, also, from the experience of others,—of its effects in various diseases, and laid it before the French Academy of Sciences. The committee appointed by the society to examine and report on this communication, of which the celebrated Percy was a member, reported unfavourably, stating that the accounts which had been published of its remediate powers were exaggerated and equivocal.‡ The hope, therefore, which M. Chrestien entertained of having his favourite remedy formally, and, as he imagined, honourably introduced into the materia medica, was frustrated in this quarter. To do away the discredit which the report of the committee tended to throw upon the article, was Mr. Neils' principal motive for publishing the present work. His experience with the medicine was extensive and successful; and he felt himself called on to offer his testimony in its favour, in opposition to the judgment of *Percy.*

In the introduction to this work, the author gives a cir-

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* Ars Chirurgica, p. 276. By W. Salmon, London, 1699.

† Fodere, Detouches, Odelius, Béche, Mitchell, Duportal, Plenciz.

‡ Journal Complémentaire du Dict. des Sciences Médicales—October, 1818.

cumstantial account of Mr. Chrestien's mode of preparing the *triple muriate* of gold, and of his manner of prescribing it.* He administered this salt, by frictions on the gums and tongue, in doses of one-sixteenth to one-eighth of a grain daily. The oxide of this metal he employed internally, in doses of from half a grain to two grains; and the metallic gold, in the form of very fine filings, to the extent of three grains daily. It does not appear that any peculiar advantage is obtained by using the triple muriate, in the form of frictions. It is much more conveniently, and, according to our own observation, as safely and efficaciously administered internally, either in the form of pills, or enveloped in some mucilaginous fluid. In this way we have frequently given it in doses of from one-fourth to one-tenth of a grain, without having, in a single instance, observed any unpleasant consequences to follow. In prescribing it in the form of a pill, we prefer uniting it with the extract of conium maculatum. By its narcotic powers, the conium allays the irritation which the action of the gold is occasionally found to excite in the stomach and bowels, and it, moreover, possesses no inconsiderable powers in itself, against those diseases for which gold is usually prescribed. This salt may also be very conveniently and properly administered in solution in the *compound decoction of sarsaparilla*, with the addition of simple syrup. Two or three grains of the muriate, are to be dissolved in eight ounces of the decoction, to which four ounces of honey must be added. Of this a table-spoonful is to be taken three times daily. The preparations of gold, like all the other heroic remedies, operate quicker, or slower, more violently, or more feebly, according to the age, condition, and the irritability of the individual; and, above all, according to the mode in which it is employed. According to Dr. Neil's experience, the powder of gold prepared with mercury, operates more slowly and in a less irritating way than the oxyd or the muriate; and the

* This preparation may be had in a very pure state at Mr. Stitts' apothecary store, at the North East Corner of Sixth and Race streets, of this city.

effects of the muriate are more permanent and certain than those of the oxyd. Gold, in its metallic form, although less efficacious than the muriate as a remediate agent, operates with great violence, and should be used with particular caution, especially when given to irritable subjects. Dr. Neil observes, that the auriferous preparations not unfrequently produce salivation. In the above extract from Salmon, the same effect is noticed. The salivation produced by gold, is, however, of a different character from that which is excited by mercury. It is always mild, not copious, and unattended by that peculiar fetor which distinguishes mercurial ptyalism; nor is the taste materially altered, or the powers of the digestive organs much diminished thereby. During a salivation from gold, the patient need not be confined to his room, but may be permitted to follow his ordinary occupations. Salivation from gold is not liable, like mercury, to produce painful and extensive ulcerations of the gums and the tongue. It is attended, simply, by a slight inflammation of the inner surface of the mouth, and does not weaken, but, on the contrary, it invigorates the general powers of the system. Dr. N. observes, that a cold and damp atmosphere retards the operation of gold.

Having given an account of the general effects of gold on the animal economy, our author proceeds to detail the history of a very considerable number of cases of various diseases, in which he prescribed the preparations of this metal with signal success. Among the examples which he relates, as having yielded to the powers of this remedy, we find cases of *rhagades, condylomatus excrescences, inveterate leucorrhœa, bubo, gonorrhœa virulenta, venereal ulcerations and excrescences, general syphilitic affections, rheumatism, nodes, swelled and indurated glands, venereal eruptions, cutaneous ulcerations, general scrofulous habit of body, tinea, struma and scrofulous eruptions, scirrhous tumours, with and without ulceration, scrofulous disease of the joints, irregular menstruation, chlorosis from a scrofulous habit, lymphatic swellings and abscesses in*

children, ulcers of all kinds, caries, scrofulous opthalmia, goitre and scirrhous testicle. The complaints, however, in which this remedy is found most decidedly beneficial, are those of a venereal origin. In primary venereal affections, it is proper to postpone the employment of the gold until the first and more acute stage of the disease has passed by. The author observes, also, that in old and obstinate syphilitic disorders, considerable advantage will be gained by occasionally changing the form of the medicine. Sometimes he rose rapidly with the dose, and then diminished it again to its minimum. With regard to the powers of gold in scrofulous affections, he observes, that where the disease had not been of very long standing—where it was, as yet, confined to the less essential organs of the system,—where the whole glandular system was not yet too deeply involved in disease, and the general habit not too much depraved, he found this article of great use. In affections of this kind, he sometimes employed the oxyd, and at others, the *triple muriate.* He speaks in very high terms of the utility of an ointment, made by incorporating twelve grains of the powdered gold with an ounce of lard, as an application to scrofulous ulcers. Frictions with this ointment were also occasionally made, with benefit, on obstinate glandular indurations.

Out of the great number of cases related by the author, we subjoin an account of a few of the most interesting, as examples, at once of the powers of the remedy, and the mode of managing its employment.

A man aged forty-seven, applied to Mr. Neil for relief from a distressing venereal affection. He complained of great pain in his bones, attended with so much torpor and stiffness of the lower extremities, that he could not walk. There was an exostosis on the left clavicle, and a hard indolent tumour of the size of an egg, on the forehead. The patient had had frequent venereal infections, which were never regularly treated. Mr. N. prescribed the use of the muriate of gold. The first three grains were divided into fourteen, twelve, and ten parts; one of which was rubbed into the tongue during the day. With the exception of a

slight increase of appetite, no sensible effect was produced by these doses. During the employment of the fourth grain, however, the swelling on the forehead inflamed and suppurated; the sixth grain excited a mild fever, which lasted three days, and was attended with swelling of the tonsils; to this fever a mild salivation succeeded, and continued a month. The medicine was omitted after the sixth grain. During the progress of the salivation, the pains abated rapidly; the lower extremities recovered their pliability and strength; the abscess on the forehead healed, and the exostosis, also, gradually disappeared; the patient, in short, recovered his health perfectly. The author gives another case, similar to this one, from the work of Gozzi, an Italian phycian which was successfully treated with gold.

In 1817, a gentleman consulted Dr. N., with gonorrhœa and chancre on the body of the penis. He had been treated in London with mercurial remedies, and was considered entirely well. About six months afterwards, two bubos appeared in his groins. These suppurated, under the use of mercurial frictions, by which a slight salivation was excited. Seven or eight months after this second cure, the patient began to experience pains in his bones, which, by degrees, increased to great violence. Having suffered a whole year in this way, he resolved to go to Paris for aid, and was there again treated with mecurial frictions. He derived great relief from this treatment, but the complaint did not entirely yield. Seven months after this period, the pains again returned with great violence, becoming at last excessively tormenting, especially in the back of the head. An eminent Parisian physician now recommended the employment of the *Rob de Lafecteur.** After using nine bottles of this medicine, the patient

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* The following is said to be the correct formula for making this *Rob.*

Rad. Sarsaparillæ. Lig. Guaiaci āā ℔i. Folior. Sennæ. mimosæ nilot āā ℥i. Rad. amom. Zingib. ℥ss. aq. fontanæ O. x. Boil the two first articles in water for an hour, strain, and add to the residuum the same quantity of water as before; boil it for two hours, and towards the end of the boiling add the other ingredients, strain, and to both decoctions add common sugar and honey, of each three pounds, and boil the whole to the consistence of a syrup. Dose, ℥ii twice a-day. The patient is directed to take the first dose early in the morning, and the second four hours after dinner. He is also to take, during the day, twelve small tumblers full of sarsaparilla

was so far relieved, as to remain perfectly comfortable for twelve months. At the end of this time, however, a new chancre appeared on the glands, although the patient declared that he had not been exposed to the chance of a new infection. The bulb of the urethra, also, became considerably enlarged; he had pain in the hypochondria, and acquired a sallow complexion. The *Rob* was again resorted to, but to no advantage. The patient now applied to Dr. N. who put him upon the use of the muriate of gold. The dose of the medicine was gradually increased from one-tenth to one-fourth of a grain per day. On the third day after he had risen to one-fourth of a grain, a gentle fever came on. The dose was, therefore, again reduced to one-eighth of a grain per day; after the use of eleven doses of which the febrile symptoms reappeared. The gold was now discontinued; a profuse perspiration broke out, which lasted five days, and was attended with a peculiar milliary eruption. The chancre gradually healed; the swelling of the bulb of the urethra disappeared, as well as the pain in the hypochondria, and the patient recovered his original vigour and vivacity. The author here remarks, that the preparations of gold sometimes do not evince their effects until their use has been continued for a very considerable time. Cases, in fact, occur, in which even very large and long continued doses do not produce the least reaction in the system. Dr. N. found that in many individuals the preparations of gold excited a hilarity of disposition, especially in those who were much debilitated.

A woman aged thirty, had chancre with bubo, and *fluor albus*, in her eighteenth year. About ten years afterwards she became affected with pains in the bones of her left leg, which soon acquired an excessive degree of violence, being more particularly intense during the night. A node appeared on the left tibia, and almost the whole surface of her body became covered with warty excrescences, some of which were of the size of a mulberry. The pains spread to the superior extremities, and rendered her incapable of moving her body. A slow fever supervened—attended with watchfulness, inappetency, and great despondency. In this situation, she was put upon the use of the muriate of gold. She be-

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decoction, seven before dinner, beginning two hours after having taken the first dose of *Rob*, and five after dinner, beginning also two hours after the second dose.

gan with one-sixteenth of a grain a-day, and gradually increased it to one-fourth. After the sixth day, the appetite was improved; after the twentieth, the pains were much less severe, and after the thirtieth day, they were greatly diminished;—perspiration profuse and general came on, which brought great relief. The patient became more and more cheerful; she could now rest and sleep at night. By the ninetieth day, her strength was fully restored—the catamenia, which had been surpressed for five months, reappeared—and she soon recovered a complete state of health.

The author states, that, in one instance only, did this remedy produce colic pains. In this instance, the irritation of the bowels was followed by pretty smart purging, of four days continuance. In general, the auriferous preparations have a tendency to produce a slight degree of constipation; Gozzi, however, observes, that when the doses employed in the frictions are very large, they are apt to excite pain in the stomach and bowels, and diarrhœa. We have given the gold in much larger doses than those which are mentioned by our author, and have never known any disagreeable effects to arise from it. This, perhaps, arose from our mode of employing it, which was generally in combination, either with opium or cicutæ.

In 1816, Dr. N. was consulted by a young man who had gone through a course of mercurial treatment at Paris, without obtaining any relief from it. He had an enormous phagedenic ulcer in the left groin—the consequence of a venereal bubo—which extended down close to the anus, and about four inches above the symphisis pubis. The inguinal glands were almost entirely destroyed; the edges of the ulcer were uneven, elevated, recurved, bloody, and exceedingly painful; the surface was covered with pale-bluish granulations, discharging a thin, yellowish, and very fetid and coroding pus. He had a slow wasting fever. Every local application which he had tried, only aggravated his sufferings. Dr. N. prescribed the muriate of gold internally, and the gold ointment (made by mixing one drachm of powdered gold with an ounce of lard) was directed to be applied as a dressing to the ulcer. Much relief was obtained from the first dressing;—the

discharge was lessened, and the spongy granulations subsided. On the twentieth day from the first employment of the gold, the edges of the ulcer were more regular, and cicatrization had commenced in the centre. By the forty-seventh day, the ulcer was completely cicatrized, the fever subdued, and perfect health soon re-established. Dr. N. found the gold powder (*or devisé*) an excellent remedy for venereal warts. He applied it in the form of an ointment, or mixed with spittle, once or twice a-day.

A lad, aged about eleven, affected with lymphatic swelling of one of his knees, applied to Dr. N. for advice. The disease was of two years standing; the condyles of the femor were considerably enlarged; the joint was stiff, and the leg retracted. Various remedies had been fruitlessly employed; and the patient had just returned from Paris, where he had been under the treatment of several eminent practitioners, without obtaining any relief. Dr. N. put him on the use of the muriate of gold. The first grain was divided into fifteen, and the second into fourteen doses. These produced no sensible effect. The third grain was divided into ten doses, and, by the time that these were used, the lymphatic swelling was diminishing rapidly, and the boy could extend his leg considerably. Three grains more, divided each into eight doses, were afterwards used, but the enlargement of the condyles remained undiminished. The muriate was then employed in doses of one-seventh of a grain daily; and after having taken two grains, in these doses, he felt a considerable itching in the tumor, and the swelling became smaller, and less firm. The remedy was continued one month longer, and the dose increased to one-fifth of a grain daily. This produced profuse sweating, and the patient improved rapidly. The knee, to all appearance, became perfectly sound; a slight lameness only remained, from the shortening of the leg by the disease.

A child had an eruption on the head, which was cured by pulling out the hair. Some time after the disappearance of the eruption, the glands about the neck and lower jaw began to enlarge on both sides. Suppuration took place just under the angle of the left jaw, seven months after the first appearance of the swellings. The left knee, also, now inflamed and suppurated. The abscess was opened by a surgeon. A year after the ulceration under the angle of the jaw, the ulcer was about an inch in diame-

ter, with hard irregular edges, and a very unfavourable aspect. The whole surface of the knee, also, was ulcerated; the edges of this ulcer were irregular and painful, the granulations unhealthy, and the discharge of an ichorous character. The glands of the neck still remained swollen and hard, but not painful; the abdomen was tumid; the left leg somewhat atrophic, and the patient was much exhausted and emaciated. He had a constant slow fever, attended with nightly exacerbations. The muriate of gold was prescribed, and the patient's condition soon became more favourable under its use. On the twenty-third day, his appetite was much improved, the febrile exacerbations had ceased, his strength was increased, and the ulcer on the knee looked much better, the edges being already considerably contracted. After the fifty-seventh day, he took the filings instead of the muriate. One grain, and afterwards one and a half and two grains, were daily rubbed on his tongue. After having used the medicine in this form for forty-two days, the patient's health was much improved; his strength was almost entirely restored; the ulcer on the knee was diminished to half its original size, and the disease about the neck was in a corresponding state of improvement. By continuing the use of the medicine for some time longer, the patient's health was completely restored.

These are a few of the great number of cases given by our author, in illustration of the medicinal powers of gold. From our own experience, we are induced to estimate its virtues very highly. In old syphilitic complaints, and in such as proceed from the use of mercury, gold is undoubtedly a remedy of great efficacy. We will conclude this article, with an account of some cases from our own practice, illustrative of the efficacy of this remedy.

Case 1.—A young man, about seventeen years old, had been afflicted with chancre for several weeks, which had created a large induration around its basis, and was attended with a bubo of considerable size in his left groin. He first took the blue pill three times daily, which salivated him severely, without producing any advantage. The bubo was retarded by purges, low diet and rest: but it did not afterwards subside under the use of a large vesicatory. He was then put under

the influence of the pills of muriate of gold and extract of cicuta. In eight days afterwards a manifest improvement had taken place, and in about a fortnight, both the indurated chancre and the tumour in the groin had entirely disappeared. It is now nine months since he was under treatment, and no constitutional affection has as yet manifested itself.

In the secondary forms of venereal affections, of both the first and second order of parts, gold is a still more valuable remedy. It produces a speedy filling up of the ulcers in the fauces, removes the discolourations of skin, and disperses the tumours of the bones and periosteum more rapidly, according to the observations we have made, than even mercury itself.

Case 2.—A woman, aged twenty five, had large and white excavations in her tonsils: several discoloured spots upon the skin of her neck and arms, and a prominent firm tumour on each patella, of about the size of a pullet's egg. She first took the muriate of gold with cicuta, with evident good effects; but on changing the formula to a solution of muriate in the compound decoction of sarsaparilla the symptoms remained stationary. She was therefore ordered to resume the pills of the muriate and cicuta, in which the dose of the former was increased to one-fourth of a grain, three times daily. In about fifteen days every symptom of her complaint was removed, and she has not applied for advice since. She was dismissed from treatment in September last.

Case 3.—The husband of the above woman had large ulcers on both legs of an irritable character, with deeply copper coloured skin around, and two large nodes, one on the middle of the right tibia, and the other on the opposite ulna. This patient was completely relieved in less than a month by the use of this remedy.

Case 4.—Bruce, a coloured man, had large excavated ulcers in his tonsils, which had came on about three years after the use of mercury unsuccessfully administered for the original venereal disease. He was put on the use of the muriate of gold, and in a short time the cavities were filled up, and cica-

trization took place in less than three weeks from the time he first took the medicine.

Case 5.—A gentleman, thirty-two years of age, was affected with exceedingly painful ulcers in the tonsils, uvula and gums, and on both sides of the tongue. He could only swallow the blandest fluids, and he articulated with great difficulty and indistinctness. The ulcers appeared to be of a phagedenic character, although they were generally covered with a white incrustation. They were accompanied with a highly inflamed state of the surrounding mucous membrane. The lower incisors were very loose and black, and their alveolar processes were deeply exposed, and finally extracted in a necrosed state by a pair of forceps. High febrile irritation, of a well marked hectic character attended, as well as great emaciation and prostration of strength. He had been infected with the primary symptoms of the venereal disease at three several times previously; all of which attacks had been subdued by mercurial remedies. Eighteen months had elapsed after his last infection, before the symptoms which we have described made their appearance. He had taken the blue pill, corrosive sublimate &c., without any advantage. Under these circumstances he was put upon the use of the muriate of gold, dissolved in sarsaparilla decoction, in the proportion of one-eighth of a grain to every half ounce of the latter. Considerable amendment was soon manifested, which progressed regularly for about two weeks. The symptoms then remained stationary for several days, when the bowels became costive and the stomach disordered. An emetic of ipecacuanha, followed by a dose of rheubarb and magnesia, corrected these symptoms; and the gold again was resorted to in larger doses. The ulcers again improved, and finally, under the use of the same remedy, combined with cicuta, they were completely healed in less than five weeks from the commencement of the treatment. The general health and appetite of the patient, at the same time, improved, so that he was enabled to return to his occupations sometime before his cure was effected. He

discontinued the use of all medicine in November last, since which time he has remained perfectly well.

Case. 6.—A young man, aged nineteen, applied to us with an enormous enlargement of the lymphatic glands of the throat. They formed an exceedingly large tumour, which extended underneath the jaw from one mastoid process to the other. Two abscesses had previously ruptured and discharged a scrofulous matter. His general health appeared to be good, and his appetite was unimpaired. He was put on the use of the gold and cicuta pills. After having taken them for about a week, the tumour rapidly diminished, and in about twenty days the several enlarged glands which were matted together into the common mass, became insulated from each other. They afterwards decreased very slowly, and although greatly diminished, they have not yet entirely disappeared.

Case 7.—A young lady, aged twenty years, had enlarged scrofulous glands on both sides of her neck, in the left axilla, and under the clavicle of the same side. She exhibited all the appearances of a scrofulous habit of body. But the symptom which alarmed her family, and induced her to apply for medical aid, was the appearance of an induration of considerable size in her left mamma. As this tumor was slow in its progress, and gave little or no pain, we supposed it to be of the same character with the scrofulous glands in her neck. She was put upon a farinaceous and milk diet, and the gold and cicuta pills were prescribed for her, in October last. In four weeks the glands in her throat and axilla had almost entirely disappeared, and the tumour in her mamma had greatly subsided. The remedy was then discontinued, under the idea that it had made a sufficient impression upon the constitution. We saw her a few days ago, and there was hardly a vestige of the tumour remaining.

Case 8.—Miss B. was covered, in January last, with large herpetic blotches, of a blood red colour, which occasionally desquamated, disappeared, and again returned. Her case was pronounced, by a respectable practitioner of New York, to be a genuine leprosy. She took the gold and sarsaparilla about

four weeks, and was completely cured. It should be stated, however, that the use of gold in this case was followed by the arsenical solution, which assisted, in no small degree, to confirm the cure.

Case 9.—We were consulted by Mr. Wood, in Race, above Tenth-street, for advice in three cases of *porrigo circinata*, in his family. The youngest of the patients was about two years old, the second five, and the third about seven. We at first employed various local applications, recommended in such cases, but without any permanent advantage. The gold was finally prescribed. One grain of the muriate was dissolved in an ounce of water, to which an ounce of simple syrup was added. Of this the youngest took twenty drops, and the others, a small teaspoonful, three times daily. The white precipitate ointment was applied to the diseased spots once or twice a-day. In the course of about four weeks from the time they first took the gold, the disease in the three little patients was entirely removed.

Case 10.—Mr. Mulloch's child, in Lawrence-street, was affected with an eruptive disease of the scalp, which we considered as belonging to the variety of *porrigo favosa.* A bound girl in the same family was affected with the same disease. The gold was given in these two cases, in solution, as in the above instances. In Mr. M.'s child, the disease disappeared in about three weeks, but in the case of the bound girl, no impression whatever was made on the disease, although the medicine was continued a very considerable time. No local applications were made in these instances.

Case 11.—We were called, last fall, to a daughter of Mr. Middleton, back of No. 300 Race-street, aged about three years. Her gums were in a state of extensive ulceration, of a gangrenous character, which had come on without any assignable cause. The incisors of the lower jaw had come away, and the upper part of the alveolar processes were exposed and black. There was considerable irritation of the general system; but the child did not appear to suffer much pain from the ulceration. The ulceration in the soft parts was progress-

ing rapidly, and there was every appearance of a speedy fatal termination. The gold was prescribed in solution, as in the last two cases, and the affected parts were moistened with a weak solution of nitrate of silver in water. The disease of the soft parts was somewhat checked in about four days; but the necrosis of the lower jaw was evidently extending. In about eight days from the commencement of the treatment, the ulceration of the gums was obviously arrested, and in three weeks the soft parts appeared to be almost entirely healed. The gold was, however, still continued; and it was soon found that the whole lower maxillary bone had lost its vitality, and was about to be separated and cast off. It was gradually pushed upwards, and separated from the adjoining soft parts, so as to stand exposed above the inferior lip in the front part of the mouth. By degrees, it became entirely loosened from all its attachments, and might have been readily taken out whole with the slightest force, if the opening of the mouth had been sufficiently large to admit of its extraction. As the front of the bone rested on the inferior lip, and projected a few lines beyond the surface of the mouth, it was readily divided with a small saw at the symphasis; after which the two branches of the bone were taken away with the fingers without the slightest hæmorrhage or pain. With the exception of the right condyle alone, the whole of the bone came away. The child soon recovered its health perfectly; the chin is considerably retracted, but the deformity is much less than might have been expected. E.

ART. III.—*Eilfter Jahrsbericht des Kœniglichen Poliklinschen Institutes der Universituet zu Berlin; umfassend die Jahre*, 1820—21—22. Von DR. C. W. HUFELAND, and DR. E. OSANN. Berlin, 1823. Octavo, pp. 101.

Eleventh Annual Report of the Royal Polyclinic Institute of the University of Berlin, including the years 1820—21—22, *&c. &c.*

Attached to the University of Berlin, is a charitable clinical institution, to which the poor of that city have free access for

medical aid, either as out-door or in-door patients. During the years 1820—21—22, there were 2447 patients treated in this institution, of which number 1754 were cured, 168 removed to other institutions, 68 died, and 118 remained under treatment.

There is much interesting matter in the present Report, and what adds to its value, it is drawn up with great neatness and method. We proceed to give our readers a condensed account of its contents.

Fever.—The prevalent character of the febrile diseases treated in the Polyclinic Institute, during the three years included in the Report, was of the inflammatory. Gastric and rheumatic fevers were of the most frequent occurrence. In these forms of fever, the muriate of ammonia, so much recommended by Dr. Hoffman, was found a very useful remedy. It was given, either dissolved in Vin. Antimonii, or in some mucilaginous liquids, and, occasionally, also in combination with laxatives. The German physicians, in general, prefer the muriate of ammonia to nitre, as an antiphlogistic, in febrile affections. Its disagreeable taste is almost entirely covered by being dissolved with the extract of liquorice in water. The sulphate of quinine was given in some cases of intermittents, with very prompt relief. In a few cases of typhus fever, complicated with local inflammations, much advantage was obtained from topical bleeding, by means of leeches, and from ice-cold applications over the affected parts. In these cases, also, the *chlorine* was found particularly efficacious. Diluted in water, and mixed with some mucilaginous substance, it was readily taken, and used for a long time, without disordering the stomach, or producing any irritation in the glottis. It sometimes increased the alvine evacuations. Where there was a disposition to diarrhœa, the acid was given in combination with infusion of *Colomba.*

Exanthemata.—Scarlet fever did not occur often, and was in general mild. Under a moderately exciting *regimen*, the disease went regularly and favourably through its course; but under a very heating one, it was apt to assume a malignant

character. In some cases, attended with unusually severe anginose affections before the appearance of the eruption, much benefit was derived from the application of leeches to the neck. Several cases occurred where the eruption receded soon after its appearance, and was followed by alarming symptoms of inflammation of the brain. Local bleeding, together with the internal use of musk, calomel, and the precipitated sulphuret of antimony, appeared to do much good in such cases. *Measles* prevailed epidemically at Berlin during the last year included in this report. The epidemic was of a mild character, and did not offer any thing unusual either in its symptoms or complications. In a few cases of metastasis to the lungs, relief was obtained from the application of blisters and leeches to the thorax, and the use of calomel, digitalis and musk.

Chronic cutaneous diseases.—These affections, it is observed, are to be viewed, in the majority of cases, as symptomatic of a general and more intimate affection of the animal economy. Prescribing with this view, the remedies were chiefly directed against the radical affection, and the result was in general very favourable. A child, affected with *petechiæ sine febre*, depending on a scrofulous habit and intestinal worms, was speedily cured of its cutaneous disease, by remedies directed against these causes. In herpetic eruptions dependant on a scrofulous dyscrasy, great advantage was derived from the use of *Plummer's powder*,* *g. guaiac* and the external employment of the *white precipitate ointment.* Among the external means of relief in obstinate herpetic affections, scarifications deserve to be particularly mentioned. In eruptions depending on a syphilitic taint, perfect relief was always obtained from the internal use of *muriate of mercury, g. guaiac.* and *dulcamara.* The *calx antimonii*

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* ℞. Sub-muriat. Hydrarg.
Sulph. Aur. Antimonii āā ℈i.
Sacch. Alb.———— ʒii. M. dose from eight to ten grains for an adult.

sulphurata, and *antimonium crudum*, were employed with much success against scabies, and anomalous eruptions depending on a rheumatic cause.

Phlegmasiæ.—Urgent, as was, in general, the necessity of a strictly antiphlogistic treatment in the phlegmasial diseases, instances of these affections occurred, which, by a rapid and unexpected transition from the inflammatory to the typhoid state, rendered the employment of the most potent stimulants necessary. Cases of this kind commonly terminated in death. The old practice, lately brought forward again, by Peschier,* of employing large doses of *tartar emetic* in inflammatory affections of the breast, with a view to its antiphlogistic or sedative effects, was frequently resorted to, with the happiest results. In fifteen cases of this kind, treated in the Polyclinic Institute, from half a grain to a grain of this article was exhibited every two or three hours, with unequivocal advantage. In a few cases, only, was it necessary to discontinue the *tartar emetic*, in consequence of its too violent action on the stomach and bowels. This method of treatment was especially efficacious in the primary stages of rheumatic, rheumato-gastric pneumonic inflammations. In a number of cases, the *tartar emetic* rendered the use of bleeding unnecessary, and speedily effected a favourable change in the disease. Not less useful was this remedy, in the second stage of pure sthenic inflammations of the breast, after one or two bleedings. It had the effect of subduing the remaining inflammatory diathesis, and of promoting the expectoration, as well as the cutaneous and venal secretions.

Pulmonary Consumption.—The number of patients suffering with this disease, who resorted for advice to this institution, was very great. With regard to the relative frequency of the disease in persons of different occupations, it was found, that weavers, (seidenwirker) stonecutters, masons, carpenters, and persons accustomed to play on wind instruments, were most subject to it during the three years included

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* Bibliotheque Universalle. Genevé, Juin, 1822.—See our Analecta.

in the report. During this period there was not a single case of the disease observed in a butcher. This corresponds with the observation of *Withering*, that butchers are least subject to phthisis. *Pearson* has noticed the same fact. Besides the pernicious influence of particular occupations, rheumatism, and anomalous hæmorrhoidal affections in men, irregularities of the catamenia in women, imperfectly cured pulmonic inflammations and neglected catarrhs, were the most usual occasional causes of this disease. *Prussic acid*, though certainly too much extolled by some, was much used in the form of *aqua lauro-cerasi* in combination with *digitalis* in *phthisis florida*, and generally with considerable advantage, though in no instance with complete success. Besides these remedies, issues, repeated small bleedings, marubrium, millefolium, &c. &c. were occasionally employed, with benefit.

Diseases of the Heart.—Various cases of dynamic affections of the heart, depending, sympathetically, on irritations seated in other organs, were treated at this institution.

A lad, about sixteen years old, of a full habit of body, suffered much from a pressing pain in the left side, attended with violent palpitations of the heart, and was much subject to walking in his sleep. Repeated bleedings, digitalis, aqua lauro-cerasi, and purging with sulphate of soda, cured him of his disease.

A girl, aged sixteen, who had not yet menstruated, suffered from her fifth year, (at which time she had a severe attack of pleurisy) with constant palpitation of the heart, attended with headache, and occasional spells of fainting. The distress in the breast always increased towards evening. The pulse of the left arm was fuller and stronger than that of the right; and she frequently experienced a numbness and tingling sensation in the extremities of the fingers. As all the other symptoms indicated a general plethora, depending, probably, in a considerable degree, on the retention of the catamenia, she was put upon an antiphlogistic course of treatment, which had the effect, at once, of establishing the regular flow of the menses, and of freeing her of her disease.

Tussis Convulsiva.—Of all the means resorted to in the treatment of this disease, *Belladonna* proved the most decidedly advantageous. In one instance, only, did this article produce *narcotic* effects; and these were not violent, and soon disappeared on discontinuing its use. To a child about twelve weeks old, affected with hooping-cough, an eighth of a grain of the powdered root of belladonna was given twice a day, with prompt and decided benefit. In the atonic stage of this affection, very great advantage was derived from the use of the *lichen islandicus* in decoction. In some instances, where the belladonna had greatly mitigated the spasmodic character of the disease, but where the cough and blennorrhœa still continued, notwithstanding its further use, the cure was speedily completed by the employment of the *lichen.* In one instance, a rapid cure was effected by the tartar-emetic ointment rubbed on the breast, so as to produce a copious pustular eruption. The *aqua lauro-cerasi*, proved very effectual in a case of this disease in an adult. Besides these remedies, the *extract. lactucæ*, as well as the *lactucarium* of Drs. Coxe and Duncan, was given in this affection, with various degrees of advantage. The *lactucarium*, obtained from *lactuca scariola*, was given to two children, severely affected with this disease, with remarkably prompt and permanent relief. One of these children was four, the other six years of age. Two grains, rubbed up with sugar, was given to each, in divided doses, daily. In six other cases, however, which were afterwards treated with the lactucarium, obtained from the *lactuca scariola*, as well as with that extracted from the *lactuca sativa* and *l. virosa*, no improvement was effected in the disease. The *extractum cannabis* was given in one case, with speedy and complete relief. The patient was twenty-one months old. Four grains of this extract were at first given during the day, which was afterwards gradually increased to twelve grains. No unpleasant effects arose from its use.

Arthritis.—In acute arthritis, besides antimony and nitre, emetics were frequently given with very good effect. Con-

siderable advantage was also derived from the use of *aqua lauro-cerasi*, combined with *extractum aconiti*, or spir. mindereri. In chronic cases, on the contrary, *g. guaiac. ext. aconiti* dissolved in antimonial wine, *corrosive sublimate* with opium, liquor ammonii hydro sulphurati, and similar remedies, were employed with considerable advantage. In several obstinate cases of atonic arthritis, the internal use of *petroleum* was of unequivocal service. In some incipient paralitic affections, depending on an arthritic habit, great benefit resulted from the internal use of the following mixture. ℞. Tinct. rhois toxicodendri ʒiv T. guaiaci ammonat. Tinct. aconit. æther. āā ʒiv. M. Of this twenty-five drops were given every three hours. In some cases, the dose was gradually increased to forty and fifty drops. Together with the use of this prescription, frictions with Tinct. Cantharid. were made on the affected part.

Scrofula and Worms.—In the scrofulous affections of children, most benefit was derived from the milder preparations of antimony and mercury. Goitre is not a disease of frequent occurrence in Berlin and the surrounding country. *Iodine* was used in two cases of this affection only. In one of these, the *kali hydrothiodinicum*, (hydriodate of potash) was employed in the form of an ointment, made by mixing half a drachm of this substance, with an ounce of lard, of which the size of a hazlenut was rubbed into the tumor twice a-day; in the other, the iodine dissolved in alcohol, was exhibited internally. In both instances, considerable relief was obtained. In the latter case, it became necessary to omit the medicine, several times, in consequence of the violent excitement it produced, even in moderate doses, in the nervous system; the strong congestions in the head, and the distressing constrictions it caused in the throat. In the verminous affections of children, which are exceedingly common in Berlin, the *seminæ santonicæ* in combination with *rad. filic. maris* and *limatura stanni*, were generally found most efficacious. Several cases of tape-worm were cured by the male fern.

Cephalalgia and Vertigo.—Maria H——, aged about forty, had suffered for several years with excessively violent periodical pains in the head. The paroxysms of pain were so severe that they often caused syncope and spasmodic affections resembling epilepsy. The catamenia were regular. Evident symptoms of general plethora having been present, she was put under a strictly antiphogistic treatment, and by repeated general and local abstractions of blood, together with the continued use of refrigerents, a perfect cure was effeted.

Louisa K——, aged twenty-five, of a delicate and nervous constitution, having unexpectedly received word of her father's death, became immediately affected with vertigo, followed by violent spasmodic affections of the respiratory apparatus, constrictions of the throat, and insensibility. From that time, she was subject to similar attacks, coming on, occasionally, several times in a day. All the other functions appeared to go on regularly. She was treated by gentle laxatives, stimulating pediluvia, and cold applications to the head, together with the internal use of *Valerian*, Fol. aurant. and *æther*. Under the employment of these remedies, she recovered her health.

Various cases of vertigo, without headache, but frequently attended with a tremor of the extremities, and some degree of hallucination, occurred in the practice of the Polyclinic Institute. Excessive stimulation by ardent spirits, visceral obstructions, and anomalous hœmorrhoidal affections, appeared as the principal causes of this disease.

Frederic B——, a shoemaker, aged fifty-six, was brought to the Institute, December 4, 1822. He was affected with a constant strong trembling of his limbs, great restlessness, visual illusions, especially by night, and the most distressing anxiety of feeling, amounting sometimes to the deepest despondency. His conversation indicated, by times, an approach to insanity. The patient had already suffered a long time with these symptoms, though in a less violent degree, and was subject to slight hœmorrhoidal affections. The functions of the digestive organs were considerably affected. There was a total absence of appetite, and the tongue was thickly covered with fur; nausea and occasional vomiting was experienced in the morning. Infusion of *valerian*, in com-

bination with *extract taraxaci*, extr. hyoscyami, and Aq. menthæ; together with *crem tartari* and *lac. sulphuris* as a laxative, were at first prescribed. Afterwards, the valerian alone was given, with the daily use of the following pills: ℞. *Extr. aloes, gr. xv. Extr. gratiolæ* ʒss. *Extr. taraxac. Lac. sulph. Sapon. venet.* āā ʒi. *M. f. pill pond. gr. ii. from three to six of which were taken daily.* By the continued use of this prescription, several copious and bad-looking stools were procured daily; the appetite and digestion became gradually better; he acquired strength, and by the thirteenth of January he was discharged, with his health restored.

In a case of *vertigo caduca* of a woman, aged forty-one, which appeared to depend on menstrual congestions, and on anomalous arthritic affections, very great benefit was derived from a powder composed of *g. guaiac, lac. sulphuris* and *crem. tartar.* The same remedy was also used successfully in a man aged fifty-two years, affected with this disease, depending on hœmorrhoidal congestions and an arthritic disposition of the system.

Dropsy.—In the treatment of this disease, *rad. levistici, fol. digitales* and *calomel*, appeared to do most good.—Several cases are mentioned in which a decoction of the rad. levist. with small doses of the powder of digitalis and calomel in combination, proved speedily effectual.

Diseases of the Uterus.—Several cases of Scirrhus and Carcinoma Uteri were treated at the Polyclinic Institute. *Iodine* was used in three cases of this frightful disease. In two of these the iodine was given internally, in the form of a tincture, made according to Coindet's prescription.* In the other, it was employed in the form of an ointment,† by external frictions. In every instance, the specific and powerful effects of this medicine were observed. When given inter-

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* Hydriodat. potassæ, gr. 36. Iodine, gr. 10. Aquæ distillat, ʒi. m. dose from six to ten drops.

† Hydriodat. potassæ ʒss. Axungiæ ʒiss. m. ft. Ungt. The size of a nutmeg is to be rubbed in at each twice a-day.

nally, although in moderate doses, it produced, besides other feelings, a sensation of painful pressing down and straining in the uterine system. When rubbed upon the lower part of the abdomen, it manifestly increased the disposition to hæmorrhage. Two of the patients were evidently much relieved by this medicine; but remained under treatment, when this report was drawn up. In the third case, in which the iodine was used both internally and externally, together with Herb. *cicuta* and *folia lauro-cerasi*, the disease appeared to be entirely removed.

The following case of *scirrhus uteri*, in which the *iodine* was successfully employed, is related in the appendix, by Dr. Klaproth.

Wilhelmina F., aged twenty-five, had, during her childhood, laboured under scrofulous affections, which, however, disappeared at the period of puberty. The catamenia came on in her thirteenth year, and continued to recur regularly, and without any particular disturbances in the system. She became pregnant in her seventeenth year;—Parturition was very severe, requiring the aid of the forceps. In the twenty-second year she suffered a miscarriage, which was followed by a considerable hæmorragia uteri, of four days uninterrupted continuance. Four months after this, a second hæmorrage came on, and continued eight days, by which the patient was exceedingly exhausted. From this time on, the menses ceased to return, and a continued mild leucorrheal discharge took place. In her twenty-fourth year this discharge acquired an acrid and offensive character, and was attended with a continued violent burning pain in the lower part of the abdomen. On a more careful examination, it was found that the os tincæ and neck of the uterus were enlarged and scirrhus, and very sensible to the touch. The *tinct. iodin.* was now prescribed, of which she took eight drops three times daily. In addition to this, an ointment composed of ungt. digitalis, ungt. cinereum, extr. belladon. and ol. hyoscyam. coct. spread on a piece of spunge, was applied to the os tincæ per vaginam. After the patient had gradually increased the dose of *iodine* to twenty-eight drops, syncope, violent arterial action, watchfulness, and an uncommonly excited state of the nervous system took place.

The dose was therefore diminished. By this time the discharge from the uterus had become much less corroding, and diminished in quantity. Menstruation also occurred, though weak and irregular. The dose of the *tinctura iodinæ* was again increased, until she took twenty-three drops three times a day, and it was again found necessary to lessen it. By continuing the employment of the medicine, the disease, at the time the case was drawn up for publication, had almost entirely disappeared—only a slight enlargement and hardness could be felt per vaginam.

Chorea St. Viti.—This disease was of very frequent occurrence at the Institute. It was generally found connected with the process of evolution immediately preceding the period of puberty, and also with worms. *Zinc*, particularly the *cyanite of zinc*, was used with the greatest success.

Augusta D., a girl aged sixteen, who had not yet menstruated, was admitted into the Polyclinic Institute. She had been treated for chorea, at the institution, eight years before. When admitted this last time, she laboured under a violent attack of the disease. Pills, composed of *g. ammon. ol. valerian, extr. taraxac. and flor. zinci* were prescribed; the use of these was alternated with that of the *pulvis cærophor.* The continued use of these remedies brought on the menses, and gradually removed all the spasmodic symptoms under which she laboured.

Two cases of this disease, apparently depending on irritation from intestinal worms, were cured by anthelmintic remedies, and small doses of flor. zinci, combined with absorbends. A number of worms were discharged in both instances.

In the appendix to this volume, Drs. Rosenstiel and Herkenroth report two cases of *chorea* which were successfully treated by the cyanite of zinc.

1. Emelia M., a very delicate child, aged seven, had suffered slight spasmodic affections from the first period of dentition. In the fourth year, according to the information received from her mother, she became affected, in consequence of a fright, with spasmodic twitches of the upper extremities, and the muscles of

the face, which, however, did not continue long. In the sixth year, she suffered an attack of *nervous fever*, from which she recovered slowly. In the month of February of the present year (1822) the spasmodic movements of the arms and muscles of the face returned, without any evident exciting cause, and gradually increased both in violence and extent, until the affection assumed the character of a perfect case of *chorea*. Debility, and a morbid sensibility of the nervous system, appeared to be the principal circumstance upon which the disease depended. There were also some reasons for suspecting the existence of intestinal worms. The indications of cure were, therefore, to remove the irritation in the alimentary canal, and to give strength to the nervous system. To fulfil the first indication, two or three teaspoonfulls of the *electuar anthelmintic.* Ph. P. were given daily. To answer the second indication, one grain of *flor. zinci* was ordered to be taken three times a day. The diet enjoined was simple, but nourishing. No worms, however, were discharged by the use of the electuary, and, as the indications of their existence were of a doubtful character, the vermifuge was omitted. The treatment was continued with the *flor. zinci;* the doses of which were gradually increased, until as much as grs. x. were taken during the day. Not the least relief was obtained from this treatment. This remedy was therefore also discontinued, and instead of it, the *cyanite of zinc* was given, in doses, at first, of one-third of a grain twice a-day. This, also, did not produce any favourable effect. The spasms increased,—the motions became more violent, and *aphonia* and *dysphagia* now increased the distress of the patient. The doses of the *cyanite of zinci* were then gradually increased; and the child was bathed in an infusion of *rad. calami aromat.* By continuing these remedies until fourteen grains of the cyanite were taken daily, an almost total abatement of the symptoms had taken place. The little patient could now speak again, swallow without difficulty, and exercise a full command over the motions of her extremities. To confirm the cure, she continued the use of the medicines for some time longer; it was, however, found necessary finally to omit the zinc, in consequence of the total loss of appetite, and the excessive nausea it occasioned.

Ludwig S., a boy aged twelve, had been healthy up to the time

of his attack with this disease. The complaint came on at once with great violence. Impelled by a continual internal feeling of restlessness, and a ceaseless convulsive mobility of his whole body, he ran and sprung about the room, over tables and chairs, and endeavoured every where to climb up, like a wild animal. This state of activity continued unabated during the night. His intellectual powers appeared to be nearly destroyed.—He seldom understood what was said to him. In this state he was admitted into the Polyclinic Institution. He was treated with *anthelmintics* and *flor. zinci;* and by their employment alone, was relieved of his disease in the course of about three months. About a year after this attack he was again taken with the disease, and again cured by these remedies. In about four months after this second attack, he experienced a third one, in all respects similar to the first. Anthelmintics, and the *flor. zinci* were resorted to, as in the previous attacks, but although the latter was given in as large doses as could be ventured, not the least amendment was effected. The following pills were now employed, and the result was exceedingly favourable. ℞. *zinci hydrocyanic. gr. xv. rad. valerian.* ℥ss *extr. valerian. q. s. ut fiant pilul. №.* 60. Dose, two every morning and evening, to be increased gradually. By the time that the dose was increased to five pills every morning and evening, evident amendment in the disease had taken place. The medicine was continued, increasing it gradually until he finally took eight pills twice a-day, and all the symptoms of the disease had disappeared. The pills were given about four months. E.

ART. IV.—*A Practical Essay on Typhous Fever.* By NATHAN SMITH, M. D. Professor of the theory and practice of Physic and Surgery in Yale College. New York. BLISS & WHITE, 1824. Octavo, pp. 88.

THIS small work proceeds from one of our most enlightened and experienced physicians. It is a plain, sensible, and instructive exposition of the author's views and experience concerning the nature and treatment of typhous fever—a disease, respecting which, medical opinion is as yet exceedingly unsettled, and, in many respects, unsatisfactory.

Dr. Smith observes, that the typhous fever of Europe, and particularly of England, is doubtless similar to the disease known in this country by the same name. We do not question the general correctness of this observation; but we are, nevertheless, persuaded that the term *typhus*, has, of late years especially, been very often greatly misapplied, both by the physicians of this country and of Europe. It is no uncommon thing for an ordinary continued fever, arising from atmospheric vicissitudes, to be dubbed with the name of *typhus;* more especially if, after a stout struggle on the part of the physician to subdue the disease, by what is called a *bold* practice, the patient sink into a state of irrecoverable prostration. We have known physicians to go to work upon such cases, and to purge and bleed—and bleed and purge, with the utmost industry, until subdued nature sunk, and then, forsooth, the disease was called typhus.

Typhous fever is very generally thought to be peculiar to the cold seasons of temperate climates. There is, however, much respectable testimony directly opposed to this opinion. Dr. Smith informs us that he has seen it "attended with all its characteristic marks, in every month,—nay, he might say, in every day of the year." Ferriar* and Wedekind,† whose opportunities of observation appear to have been very extensive, testify to the same fact. Certainly, this disease is, comparatively speaking, of but rare occurrence during the warm season of summer. Its infrequency, however, during this season, does not, we apprehend, depend on any power which a high degree of atmospheric temperature may possess, to destroy the contagion of typhus, but on the absence of those circumstances which favour the evolution of the contagion, in consequence of the freer ventilation of the habitations of people during this season than in winter. The warm and dry air of summer is also much less favourable to

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* Med. Hist. Vol. I. p. 78.

† Einige Blicke in die lehre von dem Ansteckenden faulen nervenfieber. Darmstadt. 1814.

the action of this morbific cause, than the cold and damp atmosphere of winter.

The contagious nature of typhous fever is scarcely questionable. Dr. Smith relates some cases in reference to this point, which place the affirmative of this question in a very strong light.

"A young man, says the Doctor, a pupil of mine, was attacked with typhous fever, from which he recovered with difficulty. Some of his family, who lived about forty miles distant, came and took care of him during his illness. Upon his recovery, they returned home in good health, but soon after sickened with the same disease, and communicated it to others who had not been exposed in the first instance. From this it spread to numerous other families in the vicinity, who had been exposed to the contagion. In the whole town where this occurred, there had been no case of typhous fever for many years, till brought there by the circumstances just related."

"During the prevalence of the typhous fever in Hertford, (Vt.) a woman went there from Chelsea, about ten miles distant, to visit and administer to a sister, sick of this disease. Others of her family contracted it from her, and in about four weeks there were thirty persons taken down with typhus, all of whom had been exposed to the contagion."

"A young man belonging to Plainfield, (N. H.) who had left his friends and resided for some time in the western part of the state of New-York, returned to his father, who had a numerous family. He found himself unwell before he reached home—was immediately confined with typhus, and soon sunk under the disease. In about four weeks after, I was called into the family and found nine members of it sick of the same disease."

The German writer already quoted, states, that during the late campaigns of the French against Russia, the typhous contagion generated in the hospitals and houses crowded with prisoners and the sick, was communicated to the inhabitants along the road by which the soldiers returned,—and that it afterwards spread gradually from the road-side to the adjacent districts, until the disease became widely epidemic. The

route of the returning army from Poland to Germany, and thence to the centre of France, could be distinctly traced by the desolating train of disease it left behind.

Evident, as it must appear, that typhous fever, under favourable circumstances, is propagated by contagion, it is nevertheless probable that it may also originate from causes of a different character. Errors in diet, the influence of atmospheric vicissitudes, marsh miasmata, and confinement in close and crowded apartments, are enumerated as the principal causes of this kind. Believing that typhus is a disease *sui generis*, as distinct and specific in its cause and character as small-pox, measles, &c., Dr. Smith, of course, rejects the doctrine which ascribes to these accidental causes, the power of producing typhus. The fact, however, which he adduces against this opinion, is by no means sufficient to justify any considerable doubts on this head. "The absence," he says, "of typhus in a large section of country, for an interval of more than twenty years, would lead us to doubt the possibility of its being produced by any of the accidental causes above enumerated; for in such an extent, and among so many people, it is impossible but that some of these circumstances should have occurred—and the disease of course be produced." The insufficiency of this argument is rendered manifest by an observation which the Doctor himself makes, in another place, in relation to *intermitting* fever. "On the Connecticut river, he says, from Northampton in Massachusetts, to its source, a distance of more than two hundred miles from north to south, and on all its tributary streams on both sides, for a hundred miles in width, there has been no instance of any person's having contracted the intermitting fever from the first settlement of the country to the present time." By a parity of reasoning, therefore, we might be led to doubt the possibility of marsh miasmata to produce intermitting fever; for as it would seem impossible that, during so long a time, and throughout so extensive a tract of country, marsh miasmata should not be generated, and consequently intermitting fever be produced, miasmata,

it might be inferred, cannot be a cause of this fever. There is, in truth, a great deal of mystery connected with this subject. We are utterly unable to give any satisfactory explanation of these phenomena, and for the present, we can only view them as inexplicable exceptions to what observation and experience has led us to consider the general course of nature.

That typhous fever, however, is not exclusively the production of a *specific contagion*, appears quite probable. We are not, however, disposed to believe with Dr. Good, that "any of the *ordinary causes* of fever may be a cause of typhus;" nor do there appear to be sufficient grounds for the opinion advanced by Armstrong, that marsh miasmata are a cause of this fever. Typhoid is evidently confounded with typhous fever by those who ascribe the latter disease to such an unlimited diversity of causes. The causes which, besides contagion, seem to be capable of producing typhus, are those which give rise to catarrhal fever, and particularly, that species of effluvium which has been aptly called ideo-miasma, and which contaminates the air of confined and crowded apartments. That a number of healthy people crowded into narrow and close apartments, may generate typhus,—especially if aided by a deficiency of wholesome and nourishing diet, the depressing passions, and a cold and moisture air, does not admit of a doubt.

It is in this way that it originates in our prisons, and on board of crowded ships. To give strength to the opinion he entertains of the strictly specific nature of typhous fever, Dr. Smith endeavours to show that it presents nearly the same uniformity of character as the small-pox, measles, and other confessedly contagious diseases. "Typhus," he says, "like other contagious diseases, has a natural termination, and if it does not end fatally when uninterfered with, it gradually exhausts itself and disappears; at the same time, unlike those diseases, it is not restricted in its duration to so narrow limits." With regard to this fever having a natural termination, exhausting itself, *if it do not end fatally*, the same might be said of many other diseases, which are manifestly

not contagious. That there is no particular resemblance, in this respect, between typhous fever and those diseases which are known to be produced by a specific contagion, is evident from the fact that typhus may vary in the duration of its course from about fourteen to sixty days, according to the experience of our author; whilst the specific diseases vary but little in this respect. Dr. Smith refers to another analogy between typhus and the common contagious diseases. This disease, he thinks, rarely affects the same individual twice. "During the twenty-five years," he says, since I first attended patients in this disease, and in that time I have visited many hundreds, and have witnessed its prevalence several times in the same village, I have never known or heard of its recurrence in the same person." In this opinion, however, he is not borne out by the testimony of other writers. Wedekind, who like Dr. S. considers contagion the sole cause of this disease, speaks of its recurrence in the same individual as by no means uncommon. "The majority of contagious fevers," he says, "with but few exceptions, attack the same individual but once. The contagious typhous fever, however, belongs to those maladies which may recur frequently in the same person. I have seen instances, where the same individual in an hospital, has had three and four distinct attacks of the disease. It was by no means uncommon, when the typhous fever prevailed in the hospitals, to see those, who had already convalesced, and were about leaving the house, sicken again with a fresh attack of the disease."*

It is clear, however, from the testimony of all those who have attended to this point, that the susceptibility of the system to the action of the typhous contagion, is, in general, much diminished by an attack of this disease.

Typhous fever is not unfrequently attended with symptoms indicating internal local inflammations. Such local affections do most assuredly occur in this disease; but their oc-

* Op. Citat. p. 158.

currence is by no means so common as some late writers would have us believe; nor can we regard that pathology as correct, which considers these inflammations primary, and essential to the production of this variety of fever. Those who maintain that typhus is essentially connected with inflammation of some internal organ, as, for instance, the brain, according to Clutterbuck, rest their doctrine upon the appearances which are discovered on dissection. This would at first sight appear to be the most unobjectionable evidence on such a point. For it cannot, indeed, be denied that appearances resembling those which exist in inflammation, are very generally found in some of the internal organs of such as have died of typhus. It is far from being probable, however, that these appearances are really the consequence of previous inflammation in the part in which they are noticed. "*Neque quicquam esse stultius,*" says Celsus, "*quam quale quid vivo homine est tale existimare esse moriente imo jam mortuo.*"* We are not to suppose that the appearances which are discovered in the dead body, must necessarily have existed whilst yet living. Not only may a part which was free from inflammation during life, acquire, either in articulo mortis, or immediately after the extinction of vitality, a red and injected appearance, resembling that of an inflamed surface, but a part actually inflamed during life, will frequently exhibit no traces of inflammation after life is gone. The interesting experiments of Dr. Seeds,† show, in the clearest manner, what little dependance is to be placed on the injected appearance of the capillaries of internal organs, as an evidence of previous inflammation. This physician bled animals to death, by opening the internal jugulars; and on dissection, he uniformly found the contents of the cranium and spinal canal so gorged with blood, that it might at first sight have been imagined that blood-letting would have saved the animal.

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* De Re Medicina. Præfat. p. 11.

† Med. Chirurg. Journ. Vol. VI. p. 107.

The following observations of Dr. Smith, in relation to the primary seat and character of febrile action, appear to us so rational and interesting, that we are induced to transcribe the whole passage:

"According to our late nosological arrangements, the Pyrexia or febrile diseases, affect principally the circulatory system; if so, the affections of the other functions, are the consequence of the change first induced in this system. But as all the different parts of this system are destined to perform different functions, it may happen that a disease may primarily affect one part only, and the change produced in the rest of that system, may be the consequence of a change produced primarily in another part.

"In the sanguiferous system, the proper function of the heart seems to be nothing more than to receive the blood from the veins, and throw it into the arteries, which may be considered as living canals intended to convey the blood from the heart to the system of capillaries. Here all the functions belonging to this system are performed, such as nutrition, reparation of the body, absorption, secretion, and the production of animal heat. Of course, the functions of the heart and great arteries must be considered wholly subservient to those capillaries, which, in reality, perform all the great and essential offices of the circulating system.

"As this fever is supposed to make considerable change in the action of the circulating system, the question presents itself, what part is first affected? does the increased action of the heart and great arteries cause the increased action in the capillaries, or *vice versa?*

"In cases of local inflammation, which produce symptomatic fever, it appears very evident that the capillaries are first affected; the action of the heart and great arteries is not changed till symptomatic fever is produced; and that this symptomatic fever seems to commence in the capillaries, is evinced by the paleness of the skin, and the chills with which its first appearance is accompanied.

"The analogy between the inflammatory and febrile action is so great, that we may with confidence rely on the similarity of cause.

"The paleness of the skin, and the sense of cold spoken of above, which attend inflammation, and precede the attack of fever, or the developement of those phenomena to which we usually apply the name, and which is followed by an increase of the action of the heart and arteries, I explain in the following manner: before the diseased action can take possession of the capillary vessels, the natural and healthy one must cease, unless disease is a mere increase of the healthy action, which we have abundant reason to believe is not the case. It is during this interim, that is, between the interruption of the natural action, and the complete establishment of the diseased one, that the patient feels the chill.

"Something of this kind is observed in cases of local inflammation, which still continues to extend itself. A few lines beyond the discoloured part of the skin, between that and the portion which still retains its natural tint, there is a pale circle, evidently showing that the action of the capillaries in that part is suspended.

"Upon the whole, whether we consider typhous fever as dependant on local inflammation or not, it is probable, nay, it is very certain, that like all other febrile diseases, the morbid action commences and continues principally in the capillary system, and *that the change which we perceive in the action of the heart and great arteries, is symptomatic of the disease existing in that system.*

"The most violent affections of the heart and large arteries, as in palpitation, do not produce the slightest symptoms of fever, which serves to show that these two parts of the circulating system have diseases as distinct and different from each other as their functions."

That the functions of the capillaries are primarily deranged in febrile diseases; and that it is on the peculiar action of these vessels that the peculiar character of such diseases depend, appears to us extremely probable. Mere increased action of the heart and larger arteries, does not constitute fever. Such an increased action may be produced by any sudden and active exertions of the body; but unless the capillary functions are disturbed thereby, no fever ensues. If, how-

ever, the bodily exercise be continued, until the action either of the general capillary system, or of that of some important organ, become deranged, fever will be the result. Strictly speaking, every morbific cause makes its primary impression on the nervous system; and, as the capillary system, by which is understood all that system of minute vessels in which the various secretions are elaborated, appears to be especially under the nervous influence, it would appear reasonable to conclude, that these vessels will first experience the impressions made on the nervous system by morbific causes. It is thus, that after an exposure to a cause, adequate to produce fever, the first manifest symptoms of its action are, generally, such as indicate a derangement in some part of the general capillary system. After the capillaries of one or more important organs have been thus disordered in their functions, the heart and arteries are drawn into irregular action, in consequence both of the internal congestions from the torpor of the cutaneous capillaries, and of the irritating qualities which the blood has, by this time, acquired from the previous functional disorder of one or more of the emunctories of the system.

Our author's account of the symptoms of typhus is methodical and clear, and evidently drawn from much close observation. We shall not, however, follow him in this part of his essay; and merely quote his observation concerning the peculiar odour emitted by persons sick with this fever. "There is a remarkable odour," he says, "arising from persons affected by this disease, so peculiar, that I feel assured, that upon entering a room blind-folded, where a person had been confined for any length of time, I should be able to distinguish it from all other febrile affections." We shall here introduce an observation of Wedekind, in relation to a particular appearance of the tongue in this disease, which he calls the *lingual symptom*, (Zungenzeichen) and to which he attaches a great deal of importance. This appearance of the tongue, he says, is an infallible mark of the disease's having passed from its primary stage into that of confirmed febrile excitement. "The tongue," he observes, "becomes at first of a

colour approaching to a rose-red, is smooth, and generally not coated. When, at the request of the physician, the patient puts out his tongue, a white, but quickly disappearing streak, may be noticed, at the instant it is thrust forward between the teeth; being occasioned by a slight pressure of these on the tongue as it passes out betwixt them, just as we perceive a white but transient spot on pressing a finger gently on an inflamed erysipelatous surface." The tongue, in fact, he says, is in such cases affected with a slight superficial erysipelatous inflammation. Before this symptom appears, he regards the disease as not yet contagious. He considers this appearance of the tongue as the most certain indication of the transition of intermittents, bilious remittents, dysentery, &c., into a low or torpid state.* Etmuller says that in typhus, a tumefaction of the uvula almost is universally present; and Wedekind seldom found this symptom absent after the disease had run on for some days.

With regard to the value of medical treatment in typhus, Dr. Smith holds sentiments which we cannot advocate in their full extent. No treatment, he believes, is adequate to interrupt its course, when once completely formed. "During the whole course of my practice," he says, "I have never been satisfied that I have cut short a single case of typhus, that I knew to be such." We do not know how to reconcile this observation with the facts which the Doctor himself has recorded from his own experience, in the latter part of his essay. He there gives an account of three cases of typhus, which, from the most unpromising state—without the least indication of an approaching abatement in the disease, were brought very unexpectedly to a state of convalescence in the course of twenty-four hours, by cold affusions.

But we shall speak of these cases in another place. Dr. Smith is convinced not only that we cannot cut short the course of typhous fever, but also, that "*all* powerful remedies or measures adopted in the early stage of the disease,

* Op. jam Citato, p. 56.

are liable to do harm." It does not follow, however, he observes, that because we cannot cut short its course, we should neglect to resort to remedial treatment. "In cases of the other contagious diseases, which are destined to run a certain course, as the small pox, we often prescribe early in the disease, and with evident good effect, but not with a view to stop or cut off the disease." And, on the other hand, he observes, "it does not follow of course, that this disease in all cases requires remedies, or that a patient should necessarily take medicine, because he has the disease."

It is obviously of the utmost importance to form a correct opinion upon this point. He who believes the disease uncontroulable with respect to the term of its course, will make no effort to arrest it. Should his belief be erroneous, his treatment must be perniciously deficient. On the other hand, if it be true that the disease necessarily runs on to its natural termination, it would be evidently useless, and probably injurious, to employ any other than merely palliative means. From what we have ourselves seen of this disease, as well as from the testimony of other practitioners, we have been led to an opinion contrary to that of our author on this subject. We know that Dr. Smith is supported in his opinion by the testimony of some very eminent physicians. But the weight of authority is, we think, decidedly against him. We are, however, by no means prepared to maintain that typhous fever may, *in general*, be arrested in its course. But that there is nothing in the nature of the disease, as in small pox or measles, which renders such interference necessarily abortive, we are firmly persuaded. There has been much idle controversy concerning the propriety of taking the cure of fevers out of the hands of nature, as it has been expressed, and whether it be not better to watch the movements of nature, and give succour to her sanative efforts. Here, as in most other things, the extremes are to be avoided. The physician who, in the treatment of fevers, neglects the admonitions of nature, and disregards her opera-

tions, resembles the mariner who would conduct his vessel in safety over the ocean, without attending to nature's indications of approaching storms and calms. But on the other hand, he who remains a mere passive spectator to the efforts of nature in fever, and applies feeble and inadequate remedies, under an idea that medicine can do but little good, resembles the sailor who would expect the winds to waft him to his destined port, without plying his rudder, or shifting his sails.

In no disease are there such opposite plans of treatment recommended by different physicians, as in the present one. Those who regard the disease as idiopathic, connected, essentially, with a debilitated state of the vital powers, employ, of course, tonics, stimulants, and whatever other measures may be supposed suitable to the support of the system. Those, on the contrary, who believe the fever to be symptomatic of primary local inflammation, or who regard it as a disease of general increased excitement in its first stages, look upon bleeding and other antiphlogestic measures, as the appropriate means of relief. "It is remarkable," says Dr. Smith, "that though the practice of these two sects, for such they seem to be, is as opposite as possible, each considering the other's mode of treatment as highly deleterious, yet all boast of success, and enumerate various cases which have fallen under their care, with scarcely the loss of a single patient."

Although the results of these opposite modes of treatment do not seem to differ much, yet on comparing the reports that have been published in reference to this subject, it may be seen that the superiority of success is, in general, not on the side of the bleeding practice. Dr. Mills, one of the most zealous of the phlebotomists, informs us, that out of nine thousand one hundred and ninety-five typhous patients received into the Dublin fever hospital, and who were chiefly treated by blood-letting, seven hundred and forty died, making the proportion of death nearly one in eleven. Dr. Stocker, an eminent Irish practitioner, has shown from hos-

pital documents, that the success of other practitioners who treated this disease *without* general blood-letting, was greater than that of Dr. Mills, they having lost one in twelve, whilst Dr. Mills lost one in eleven. Dr. Stocker, who treated the disease without bleeding, affirms, that in his own practice he lost but one out of ninety-six.* The observations of Dr. Smith on this head, appear to us very judicious. "That patients often recover from this disease," he remarks, "after blood-letting has been practised, is an unquestionable fact, and the inference which should, perhaps, be drawn from it, is, that the loss of a moderate quantity of blood in typhus, is not of itself dangerous, and, in a majority of cases, may be allowed with impunity. There are cases, however, in which it may be essential to the patients' safety, while in others it may prove highly injurious. The judgment and skill of the physician is necessary in each individual case to determine the propriety of its adoption, as well as the quantity which should be abstracted." He has not found that bleeding does often produce any change in the disease, or that the pulse is rendered slower thereby. The symptoms which he regards as an indication for bleeding, are "uncommon pain in the head, attended with great heat of the part, a sense of fullness and throbbing of the temporal arteries, or marks of congestion in the viscera of the thorax —such as pain in one or both sides of the chest, increased by a full inspiration." He has never seen bleeding beneficial when the pulse was very frequent.

Much has been said of late, concerning the utility of bleeding, in what Dr. Armstrong calls the congestive form of typhous fever. In such cases, it is said, "the energies of the system are either nearly extinguished, or so much oppressed as to be unable to create an universal excitement." This prostrated state of the system is *caused*, according to Dr. Armstrong, by the accumulation of blood in the veins of the internal organs. He states that on examining the bodies of

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* Dublin Hospital Reports.

persons who have died of the congestive form of typhus, the veins of the principal organs are found in a turgid state. But, as Dr. Clutterbuck very justly, though not much to the advantage of his own hypothesis, observes, "we have no right to infer the state of the vessels during life from their appearance after life has ceased." Be this as it may, it is by no means ascertained that such congestions, if they do exist, are the *cause* of the accompanying prostration; on the contrary, it would appear, we think, much more probable that they are the *consequence* of the depressed state of the vital powers. Nor are there sufficient grounds for asserting, that the internal accumulations of blood, in cases of this kind, are confined to the venous system, this being as likely to occur in the arteries as in the veins. The engorged condition of the veins after death, is no certain evidence of their having been in this state before death. It is well known, that in almost all post mortem examinations the arteries are found to contain very little blood, whilst nearly the whole mass of this fluid is found in the veins. "How a state of *venous* congestion can take place," says Dr. Clutterbuck, "combined with a deficiency of arterial action, it is not easy to conceive, nor is any explanation attempted. There are but two modes in which an unusual quantity of blood can be accumulated in the veins; one is, an additional quantity being propelled into them by an increased action of the arteries, in consequence of which the corresponding veins are generally observed to be more distended—the other is some obstruction of a mechanical kind to the return of blood to the heart. The former of these is expressly denied by Dr. Armstrong, and of the existence of the latter no idea appears to be entertained." There is an evident inconsistency between the principles and practice of Dr. Armstrong on this head. He asserts that the prostration arises from a disproportion of blood in the arteries and veins; it is accumulated in the venous system. To equalize the circulation, he directs bleeding. Now, when a ligature is put on the arm and a vein opened, nearly all the blood which flows must evidently come from the side of the arte-

ries. But, as according to Dr. A.'s hypothesis, there is already a deficiency of blood in the arteries, it does not seem likely that such an operation would prove advantageous. Bleeding certainly sometimes raises a small and oppressed pulse to a full and active one. This occurs, when the system is really oppressed, when the power of the heart is not *impaired*, but *impeded*, by an inordinate accumulation of blood in the central parts of the vascular system.

We shall pass over our author's views concerning the employment of emetics, cathartics, diaphoretics, mercury, &c. in the treatment of this fever; and conclude this article with an account of what he says concerning the utility of cold effusions in this disease. The mode in which he uses the external application of cold water, when the patient is too weak to be taken out of bed, is "to turn down the bedclothes, and to dash from a pint to a gallon of cold water on the patient's head, face and body, so as to wet both the bed and body linen thoroughly. When the linen and bedclothes begin to dry, and the heat begins to return to the surface, the water should be again applied, and in this way the heat may be kept down to a natural standard, or rather below, on the surface." Dr. Smith relates three cases, in which cold affusions, applied in this way, produced the most unequivocal good effects. The first case was that of a young man sick with this fever, and who had been bled before the doctor saw him." The fever was severe, and the symptoms increased for several days." He was put into a warm bath. "He was a little delirious before he went into it, and when he came out he was raving. From this state, he sunk in the course of the next night into a low muttering delirium, with a great degree of coma and starting of the tendons, and with scarcely ability to swallow. His pulse was irregular, but still possessed some force, and his heat was above natural. Twenty-four hours were passed in this situation, without any symptoms of amendment." The next day he was stripped naked and laid on a straw mattress, and a gallon of cold water poured over him from head to foot. The affusion was repeated as often as he began

to grow dry and warm. In the evening his pulse did not intermit as often as before, and the heat was much diminished. He was put into a dry bed and a dose of opium and musk administered to him. On the next day the affusion was repeated, so as to keep down the heat. Before night, the patient recovered so as to speak; called for more water to be thrown on him. From this period he became convalescent, and recovered without the use of another remedy. The second case was that of a strong robust man, aged between thirty and forty. "He had been sick about a fortnight: his head was not much affected, but the heat of his body was great and his thirst urgent." The doctor saw him in the evening. The patient was stripped, and about a quart of cold water sprinkled over him. "The sheet was then thrown over him, and the water applied as often as he became dry and the heat began to return. After the first application of the water, that parched sensation of the lips and mouth which urged him to drink so much, abated, and he lay the whole night without any desire to drink." *The next day he became convalescent,* and he recovered without medicine.

The third case mentioned, was that of a robust man who had been sick two weeks, when the doctor first saw him. His pulse was frequent, his heat great, and his mouth exceedingly parched. His feet were very cold. The cold affusions were applied over the head and breast, and repeated so as to keep down the heat. He became convalescent the next day, and recovered without any further medicine.

We cannot help again remarking, that these cases are in manifest opposition to our author's sentiments, concerning the possibility of cutting short the course of this disease. There is not the least reason for supposing, that in any of these instances, the disease would have terminated as soon as it did, if the cold affusions had not been applied. In the two last cases, the patient suddenly convalesced, although on the preceding day, and before the application of the cold water, there was not the least hope of a speedy amendment. If the cold affusions produced the sudden favourable change, what reason

is there for denying that the disease was cut short in its course? To us, and, we apprehend, to the majority of our readers, it appears quite manifest that our author's doubts on this head are not justified by the result of his own experience. E.

Art. V.—*Ideen zur Diagnostick. Angefangen von* J. E. Wichmann; fortgesetzt von Wilhelm Sachse, grossherzogl. Mecklenburg, Schwerinschen, Leib und Medicinalrathe, &c. Viertter band 8vo. pp. 266. Hanover.

Ideas on Diagnosis, &c. &c.

The Germans have been long distinguished for their zeal and industry in the cultivation of medicine; and, it must be acknowledged, that they have contributed their full share to the general stock of medical improvement. To those, however, who are accustomed to pursue the plain course of observation and induction, there is something exceedingly irksome and unsatisfactory in that vein of subtle and abstract speculation which runs through the majority of their theoretical disquisitions. The spirit of the Kantean school had, at one time, infused itself, intimately, into their medical speculations; and it is only of late that they are again emerging from that mist of Idealism, to follow the light of the Baconian philosophy. For extent of learning, patience of research, and closeness and accuracy of observation, the German physicians stand unrivalled. Their writings abound in valuable materials; but their theories have been too often the offspring of an unrestrained imagination, rather than of a rigid logic.

There is another peculiarity belonging to the medical writings of the Germans, which has a tendency to perplex and fatigue the mind of the reader, without, at the same time, contributing to any useful or practical purpose whatever. We allude to the minute and unessential distinctions and divisions which are so common in their pathological descriptions. This useless refinement is, however, evidently declining among them, and a spirit of experimental research, and of sound philosophical generalization, is becoming more and more preva-

lent. Among the late medical publications that have issued from the German press, there are many that might be adduced in testimony of this remark. The work, whose title stands at the head of this article, is one which deserves this praise to the full extent.

In 1794, Dr. I. E. Wichmann published two volumes on diagnosis, which were very favourably received by his countrymen; and, in 1802, he brought out an additional volume, containing much interesting matter on this important subject. The present volume, by Dr. Sachse, is intended as a continuation of this valuable work; and from an attentive perusal of it, we are led to regard it as an accession of no ordinary value. Dr. Sachse's object, in the present volume, is to give an accurate and comprehensive view of the symptoms and causes of two varieties of phthisis—*phthisis laryngea* and *phthisis trachealis*—and to establish between them a correct diagnosis. After making some general observations on *tracheal* phthisis, he gives a full and satisfactory description of the causes and symptoms of phthisis laryngea. He relates the history of four cases of this disease, from his own observations, and of one which occurred in the practice of Dr. Fabricius. The descriptions of these cases are intermixed with instructive references to the statements of other writers, particularly of Thomann and Cheyne, in relation to this form of phthisis. In the beginning of this affection, there is hoarseness, tickling in the throat, a dry cough, slight and apparently of no consequence at first, but increasing gradually in violence, becoming at last shrill, harsh, ringing and barking as in *croup*, and attended with a puruloid and fetid expectoration. In the more advanced periods of the disease, there is pain felt in the larynx on swallowing; and the author noticed, in a few instances, that the tongue was covered with a yellowish slimy coat, on the side the pain was felt, while the other side remained perfectly clean, and of a red colour. Towards the end of the disease the root of the tongue generally becomes swollen and red, and the papillæ large and prominent. Among the predisposing causes of this disease are mentioned

scrofula, syphilis, an arthritic and hæmorrhoidal habit, chronic croup and catarrh.

In the second section, the symptoms and character of *phthisis trachealis* are minutely detailed. The symptomatology of this variety of tracheal consumption was very imperfect, until the appearance, in 1820, of Cajol's *Dissertation sur la Phthisie Tracheale.* This writer, bringing together the most important facts recorded in the dissertations of Sauvé and Petit, and collating them with those which he himself observed, has drawn up a very clear and interesting description of this disease. From this, and other authentic sources, as well as from his own observations, Dr. Sachse has furnished us with a full and instructive account of this malady.

Among the varieties of tracheal phthisis, Dr. S. mentions one which arises from the pressure of encysted and indurated tumors on the upper part of the wind-pipe. He does not mention an *elongated uvula*, as a cause of this disease, although its origin from this source is not unfrequent. Dr. Physick has cured cases of this kind, simply by cutting off a portion of the uvula, and thereby removing the irritation produced by it in the epiglottis. Wiseman, surgeon to Charles II., mentions cases of this sort, which he cured by the same operation. "There happeneth," says this writer, "*an elongation of the uvula*, through the abundance of salivous humors flowing upon it, in which case, if it touch upon the root of the tongue, or upon the epiglottis, it causeth a frequent hawking, and in progress maketh a *vexatious catarrh.* Such was the case of a servant maid to a noble lady in the country. Various medicaments had been prescribed ineffectually. I being in that family, was desired to see her. Looking into her mouth, I saw the uvula hang dangling upon the root of the tongue. It was not swelled nor inflamed. I, supposing it the cause of her distemper, took hold of the lower part of it with my forceps, and at the same time cut it shorter with a pair of scissors, and she was thereby freed of her disease. Such another case I cut off in a servant of his majesty, and

some others, who were thereby freed from their distempers, when all other remedies failed.*

But to return to the book before us. The diagnosis between phthisis *laryngea* and *trachealis*, has been accounted very difficult. This difficulty is, however, much diminished by the very minute and lucid diagnostic description which our author has given of these two forms of phthisis. We annex a translation of what he says on this head.

PHTHISIS LARYNGEA.	PHTHISIS TRACHEALIS.
VOICE.	
The voice is hoarse from the commencement, and increases with the progress of the disease; it becomes weaker, scarcely audible, and often entirely extinct.	The voice is little, or not at all altered; the patient, more than the persons about him, perceives that it is less clear; he makes frequent efforts, by hawking, to remove the cause of the irritation, which he refers to where the trachea enters the chest.
The speech, although sufficiently loud when the patient begins to speak, soon becomes weaker, and does not excite cough, or any distortions of the countenance indicative of pain; nor is it attended by sneezing, or a frequent sighing inspiration.	The speech is attended with that kind of roughness which is peculiar to many persons in health, resembling a good deal that roughness of voice which drunkards are apt to have.
There is a peculiar difficulty in uttering the first words, after rising in the morning.	The voice is alike throughout the whole day. Different positions, however, by varying the pressure on the affected parts, may produce some change in it.
PAIN.	
The pain is felt in the larynx, which the patient is apt to feel and press; it sometimes extends to a short distance downwards, but more often up into the fauces.	The pain is situated in the trachea near its bifurcation; it seldom extends upwards, but frequently downwards into the bronchia, especially directly behind the sternum, but not often to other parts of the breast, unless the affected part of the trachea is of sufficient extent to propagate its irritation to the lungs.
When the disease arises from a tumor, or other cause, pressing upon or irritating the upper part of the larynx, a tickling sensation, alone, is at first felt, which, in the progress of the disease, becomes more unre-	The pain is rather of a heavy and pressing character, as if something were pressing from the breast upwards. When, however, in the latter stage some of the cartilages become dissolved, or when the disorder arises

* Eight Chirurgical Treatises. By Richard Wiseman, Vol. II. p. 36.

PHTHISIS LARYNGEA.	PHTHISIS TRACHEALIS.
mitting, violent, stinging, cutting, or as if a foreign body were lodged in the larynx.	from an external cause, the pain is severe and piercing.
The pain is increased by coughing.	In this variety also.
——————by external pressure.	—— but very little.
——————by the inhalation of irritating vapours.	——very little increased.
It is increased by tempestuous weather.	——scarcely.
——————by heating medicines.	——here also
——————by speaking, riding in carriages, and swallowing.	—It is increased by bending the head backwards, or by turning it round.
It is increased by gargling in infusion of mustard, or vinegar and water.	——not at all.
——————by breathing the cool air of night with open mouth.	——not at all.
The pain is lessened by inhaling emolient steams.	——not at all; but by bending the head forwards.
The pain seldom abates.	There are considerable remissions.

COUGH.

A sensation of tickling precedes each spell of coughing.	No tickling before the spell of coughing comes on, but a disposition to hawk.
The cough is increased by the use of irritating gargles, by inhaling vapours, impure or cold air, and by the use of acids. It is not increased by motion, if a cloth be held before the mouth.	Not increased by these things; but easily excited and increased by bodily motion or exercise.
The cough is generally violent in the morning, until something is expectorated, and is more or less troublesome during the whole day. Like spasmodic cough, it will sometimes subside for a considerable interval—and then return with increased violence, being, in this case, usually preceded by a fit of sneezing.	Slight and frequent coughing during the whole day, becoming at last so constant as to prevent sleep at night.
The cough is sounding, shrill and grave, or acute and ringing, especially during inspiration. The inspiration during cough is stridulous, as in croup or whooping-cough.	Not ringing, but deep, hoarse, and attended with a rattling noise, as in bronchitis; inspiration is not stridulous.
The spells of coughing are sometimes exceedingly violent, when excited by external irritants, such as much speaking, drinking rapidly, inflammation in the neighbourhood of the tracheal ulcer, small pieces of loose pseudo-membrane, or of cartilage. Violent attacks of coughing occasion extreme exhaustion.	There are occasional attacks of coughing of considerable violence, ending in a kind of choking and vomiting. These spells of coughing are extremely harassing, and are attended with pains in the breast.
Cough is excited in the first act of swallowing, so that the food, when	Cough is excited when the food has already descended as low as the

PHTHISIS LARYNGEA.	PHTHISIS TRACHEALIS.
the patient first attempts to swallow, is generally thrown back again. During the act of coughing, the patient is apt to put his hand to the larynx. Expectoration lessens the violence of the cough.	sternum. It seems, at first, as if the cough and vomiting were excited only by quick and careless swallowing; but in the advanced periods of the disease, the most cautious swallowing will excite coughing. On coughing, the patient puts his hand to the throat, near the breast. Expectoration brings no relief to the cough.
During the act of coughing, there is no strong motion of the bones of the chest.	There is strong motion of the bones of the chest in coughing.
The cough is less severe at night, when in bed, but is increased on rising. Towards the termination of the disease, it can only be calmed by opium.	It is increased by lying down.

EXPECTORATION.

The quantity expectorated is generally very trifling, compared with the violence of the other symptoms.	It is more abundant, partly on account of the greater quantity of pus secreted, and partly from the mass of bronchial mucus united with it. When an abscess breaks into the trachea, the expectoration suddenly becomes very copious.
Expectoration more purulent. In the morning it is thick, being thrown out in small dense masses. During the day it is frothy. In the latter stage of the disease it has a bad smell and taste.	More like saliva, ropy, mixed occasionally with opaque streaks that sink in water. (Cajol S. 62. Ob. IV. S. 33.) Corvisart's patient did not expectorate pus until within three weeks of his death. When glands are involved in the disease, the matter thrown up is sometimes brownish, mixed with a few white points.
It is seldom, yet sometimes, bloody.	Even when the expectoration is yellow, opaque and fetid, as it generally is towards the end of the disease, it continues to be mixed with a large quantity of watery, frothy fluid, out of which the pus sinks like yeast.
It is occasionally mixed with shreds of false membranes, particularly after inflammatory attacks, as has been noticed by *Portal*, *Frank*, and myself. It is sometimes, also, mixed with small pieces of true membrane.	Mixed with shreds of membrane, consisting mostly of separated portions of the lining membrane of the trachea.
It is sometimes mixed with portions of separated cartilage, having a shape corresponding with the ulcerated and thickened part of the larynx.	Mixed with cartilaginous portions, having more of an annular form. *Portal* doubts this, v. 2. p. 55. I quote in support of it Riverius, Op. Omn. p. 51. Aretaeus Op. p. 86, also Paulin.
As to the manner of expectorating; it is to be observed, that the matter	The patient refers to the part where the trachea enters into the

PHTHISIS LARYNGEA.	PHTHISIS TRACHEALIS.
does not seem to come from within the breast, but it is brought up by a kind of hawking, and with an action of the larynx, which shows it to come from this part. Thomann relates the case of a patient, *qui continuo excreavit sine tussi sputa puriforme. P.* 166—68.	breast as the source from which the matter comes.

RESPIRATION.

Respiration becomes more and more difficult, and at last is constantly performed with painful exertion, and becomes croupy. The patient is nevertheless able to lie down or to walk about, and even to ascend heights, provided he keep his mouth closed. However difficult the respiration is, the breast remains free, and can be completely filled with air, if a spasmodic contraction of the glottis does not prevent its passage into the trachea. Attacks of threatening suffocation sometimes come on, but this happens during coughing or swallowing, and ceases as soon as the food or mucus is thrown up.	Respiration is always more or less affected; occasionally, it becomes alarmingly difficult, allowing the patient to breathe only in an upright posture. These attacks come on suddenly, last some hours, but do not keep pace in their violence with the general progress of the disease; so that the patient is often less troubled with them in the latter, than in the early periods of the complaint.
The acts of inspiration and expiration suffer an interruption before they are completed.	Breathing is not interrupted by a spasmodic action of the glottis.
The respiration is whistling—always anxious—often audible at a considerable distance, and tremulous, especially during inspiration and immediately after a spell of coughing.	The sound, in this variety, is also unnatural, but more rough; rattling, as in bronchitis, and proceeding from within the chest. This rattling sound is, however, heard only during the spells of coughing, and it is then audible at a considerable distance. In the latter period of the complaint, the breathing sounds as if the windpipe were pressed upon by an aneurism.
The difficulty of breathing is continual, though somewhat abated during the day. It is particularly difficult at night, and in the morning, and when the head is thrown backwards.	Not more difficult in the morning than during the day.

At the end of the volume the author has given engravings, representing the pathological preparations of the ulcerated and thickened parts of the affected organs. E.

ART. VI.—*Essays on Phrenology, or an inquiry into the principles and utility of the system of* DRS. GALL *and* SPURSHEIM, *and into the objections made against it.* By GEORGE COMBE; with notes and additions (*by the American Editor*) comprehending memoirs on the anatomy of the Brain, and on Insanity. Philadelphia, 1822. Octavo, pp. 462.

MR. COMBE has here given us a very ingenious exposition of the doctrines of Gall and Spursheim, and we must do him the justice to confess, that he has invested them with a degree of plausibility, of which we did not suppose them susceptible. The defence of this system could not have fallen into abler hands; he has brought into view all that can, perhaps, be reasonably offered in its support, and parried the objections urged by its opponents with much adroitness.

——————Si Pergama dextra
Defendi possent, etiam hac defensa fuissent.

We cannot, however, say so much in favour of the contributions to this volume by the American Editor, who, though animated with an abundance of zeal, has adduced nothing calculated to give additional support to these doctrines. Indeed, we apprehend that the disciples of Gall can feel themselves under no strong obligations to this writer for his earnest efforts to confirm and elucidate their doctrines; for, if we mistake not, much of what he says, in the way of argument, is calculated to bring ridicule and discredit on this system. The zealous editor himself, indeed, does not seem to be without some misgivings of this kind. He bewails, with becoming sensibility, the wounds which ridicule delights to inflict on those who suffer their zeal to outstrip their understandings. To be laughed at is, certainly, not very amusing; and yet it is not always in the power of gravity to contain itself, when absurdity stares out from under the borrowed mantle of philosophy. It is not, however, our intention to laugh at the general principles of the doctrine itself, or at its advocates. The love of truth and free enquiry forbid us such merriment. But we cannot promise to observe the same decorum

towards some of the arguments employed on this subject. Indeed, we are quite satisfied that were Democrites to rise from the grave, and hear our editor rehearse a few of his "proofs," he would, as old Burton expresses it, "break the rim of his belly with laughing."

The circumstances which first led Gall to the study of phrenology, were the following: He had, in his youth, a companion who could remember *words* far better than he himself could, and this companion had prominent eyes. Another of his companions could remember, "where, in his boyish days, he had found *bird's nests*," and this person had no prominent eyes, but a most magnificent bump "at the upper part of the internal border of each eye-brow." Next, he met with a woman who also had this bump, and he learned "that her greatest pleasure consisted in travelling." Pursuing his investigations, he lit upon a preposterously proud beggar, and this beggar was furnished with a protuberance on the posterior and upper part of his head. "Thus furnished with so many proofs of the coincidences between the developement of different parts of the brain, and the display of the intellectual and moral nature of man, Gall was compelled to renounce the obscure and incomprehensible doctrines of the schools, and applied himself to the study of the primitive faculties." Having given this account of the origin of phrenology, the editor notices some of the objections that have been advanced against this system, and proves, in opposition to every thing that might be said to the contrary, that a man could not think without brains. This, and a few other points of a similar character, being settled, he passes on to the consideration of the facts, which, in his opinion, establish the existence of a *plurality of organs* in the brain. That the reader may not be deprived of the full benefit of our editor's demonstrations upon this point, we will quote his arguments in his own words. "The *analogies of nature*, (he says) and the operations of the mind, all *go to prove* the brain's being divided into different organs. Each salt and mineral has its peculiar crystallization: each kind of tree is differently organized, as

also its different parts—the wood, the leaves, flowers, and fruits. The organization of each animal is equally modified, and in the same animal each function is attached to a particular organ, as the heart to circulate the blood, the lungs for respiration, &c." Analogy is a dangerous mode of reasoning. According to our apprehension, at least, the "analogies of nature," here adduced, serve quite as well to prove that the moon is made of green cheese, as, that the brain is divided into different organs. "Each salt and mineral (he says) has its peculiar crystallization;" and this is one of the analogies which *go to prove* the brain's being divided into different organs." Thus, to be more specific, glaubers' salt crystallizes in six-sided prisms, *ergo*, the brain is divided into different organs. Again; a peach-tree is organized differently from an elm, *consequently*, the brain is divided into different organs. This is delightful, and cannot fail to dispel the doubts of the most obdurate sceptic. We may remark, by the way, that the editor's confidence in the validity of arguments upon this point, is not at all in accordance with the opinion expressed by Mr. Combe, in a subsequent part of this book. "No argument, (says Mr. C.) can be offered to prove the existence of any faculty, or the organs of it. Every faculty and organ which are stated as ascertained, have been admitted only after an extensive series of observations of concomitance between the manifestations and developements."

Not satisfied with the above cogent analogical proofs, our zealous phrenologist, the editor, goes on to exhibit the "*direct proofs*" of a plurality of organs in the brain. "These (he says) are furnished by the actions of man and animals in a state of health and disease. As respects the latter, (animals) their brains *must* differ in the *totality*, since the qualities of these animals, depending, as we have shown, on the brain, are unlike. The beaver that builds a hut, the dog that hunts, the tiger crouching for his prey, the docile elephant, cannot have the same constructed brain. The different individuals of a species never possess the faculties in the

same degree; some are distinguished by their peculiar qualities, others are stupid—and, consequently, the organization cannot be equally perfect." This most notable specimen of logic deserves our particular admiration. What can be more conclusive than this: The brains of different animals differ in their *totality*—thus the brain of a bat differs in its totality from the brain of a horse, and, therefore, the brain is divided into different organs. We have a friend at our elbow, who insists that the philosophy of *Martinus Scriblerus*, is much more to the purpose; but we think otherwise. "A bird flies, (says Martinus) because it has wings, a duck swims because it is web-footed; and there can be no question but the aduncity of the pounces and beaks of hawks, as well as the strength of the crural and masseter muscles in lions and tigers, are the cause of the great and habitual immorality of those animals."

"If (says the editor) the brain were not composed of different organs, why do we find, in the scale of beings, that it is more complicated in proportion as the faculties are multiplied." To this imposing interrogatory we reply, that it is quite certain that the brain of a human being is not constructed like that of a dog, or a goose, but it does not follow that the brain consists of a congeries of organs. If the *intimate organization* of the brain of a human being, and that of an inferior animal, were precisely similar; and if mind and its operations were the effects, solely, of cerebral excitement, then, indeed, the difference betwixt the faculties of man and an inferior animal could only be explained by admitting that the additional parts of the brain of the former furnishes his additional faculties. It is not unreasonable to presume, however, that the *organization* of the brains of different animals itself, varies essentially; and there can scarcely be a doubt, that difference of organization—not external configuration, or mere developement—is the proximate principle of difference of functions in organs.

"In our own species (continues our phrenologist) we see, at an early period, in infancy and tender age, one part of the

brain well developed, while the other only becomes so at a later period. The first, composed of the upper and posterior part, enables them to show at once most of the passions which mark the more mature man; while it is only with the gradual and more perfect developement of the anterior portion, that the intellectual faculties are displayed, and assume a controling power over the former. Here, then, in the healthy state, we have two grand divisions of faculties and corresponding organic structure marked out." This argument is raised upon false premises. The assertion that "the upper and posterior parts of the brain are more developed in infancy than its anterior portion," is gratuitous. The fact is, that in infancy the whole brain is remarkably developed relatively to the other organs of the system. It is also, during this period, softer in its texture than in after life. The circumstance that infants manifest strong passions long before they reason, is explicable on principles much more consonant with nature than those offered in the above argument. The passions of an infant are those only which are common to all sentient beings. As soon as the infant is born it is capable of experiencing sensations. Infancy, as Bichat truly says, is the age of sensations. It is in this early period of human existence, therefore, that those passions which depend on sensations are displayed in a degree disproportionate to their power of reasoning. Thus the infant is capable of the emotions of anger and joy, because it is capable of the sentiments of pleasure and pain; and for the same reasons it is capable of desire and aversion. To the infant, almost every object it perceives is either a source of pleasure or pain. Its senses are infinitely more employed than they are in maturer age. Its incitements to anger and joy, desire and aversion, are therefore more numerous than at any other period during life. But besides these passions, which are necessarily coeval with the power of feeling, the infant has no others. Avarice, love, ambition, hatred, gratitude, and benevolence, have not yet taken a place in its tender heart. These are reserved to agitate the bosom of riper age. With regard to the *intellectual* faculties, nature

has appointed a different order of things. The evolution of the reasoning powers is necessarily gradual. At birth, the infant possesses none of the materials of thought. It has the faculties of reason, but it has not yet collected those simple ideas upon which these faculties are to be employed.—As the acquisition of ideas is gradual, so must be the developement of the intellectual faculties. To us, therefore, it does not appear very philosophical to resort to cranial bumps for an explanation of the phenomena referred to by our editor in the above specious explanation.

The following argument, though not very perspicuously stated, has been much urged by phrenologists, and it certainly possesses much greater claims to respect, than any of those we have noticed. "In the same person (says he) certain inclinations, and certain faculties, are manifested with much energy, others very feebly. One man may have much verbal memory, and little reasoning power; he may be a great poet, and no general; a good painter, and no mathematician. He may be devout, and either stupid or intelligent; each has his gifts—*consequently*, a *similar mass* cannot be adapted to all these functions." The brain is, without doubt, the organ through which mind operates; and it seems, therefore, just to infer, that the intellectual operations depend, for the degree of their excellence, on the greater or less adaptation of the brain to the perfect exercise of the mental faculties. But as this adaptation of the cerebral organ to the intellect,—or, in other words, as the fitness of the material organ to the exercise of the immaterial principle that resides in it, can depend on nothing else than on its intimate organization, it is evident, that if this organ be susceptible of modification in its intimate composition or structure, of which there can be no doubt, a correlative modification in the manifestations of intellect must obtain. The brain, by a peculiarly modified organization, may allow a more perfect display of some of the mental faculties than of the others, and thus establish original diversities and peculiarities of intellectual power. It is not necessary to admit, that a brain which is

well adapted by its organic structure, for the exercise of one of the faculties of the mind, must be equally well fitted for the display of the others. We may illustrate this by an analogy drawn from the modes of action of the external senses. Thus, some individuals have a lively perception of particular odours, which to most others are scarcely, or not at all, perceptible. Whence does this come? Shall we say, in imitation of the phrenologists, that the expansion of the olfactory nerve is divided into different organs, by which the impression of different odours is received and transmitted to the common sensorium? Or shall we say, as they do in relation to the brain, that if there were but *one surface* of nerve of a uniform structure, destined for the reception of the impressions of all odours, there could be no diversity in the power of perceiving smell? For our parts, we should say, that although the olfactory expansion be the general organ for receiving the impressions of every variety of odour, yet its organization is subject to such modifications as may render it peculiarly sensible to the impressions of certain smells, and, at the same time, perhaps, less liable to be affected by others. Proceeding in his arguments, our editor, in the next place, observes, "how can we, on the commonly received principles, account for the mind, fatigued with continued attention to one subject, applying itself to another with vivacity and pleasure. The plurality of organs explains this in the same way as when the eye is fatigued with looking at brilliant colours, or varied landscapes, and almost incapable of action, the ear will still be flattered by agreeable sounds." If the editor had employed this analogy, drawn from the external senses, as he ought to have done, he would have found in it an argument against, rather than for, the doctrine he advocates. As he tells us that the mind, when fatigued by long attention to *one* subject may apply itself with vivacity to another, he should, if he meant to adhere to a correct similitude, have made a reference to the well known fact, that the eye, when fatigued by looking a long time at *one* colour, will turn to another with delight. In this respect, the mind and the organs

of vision are perfectly similar. Would it not, however, be exceedingly absurd to say, that the retina must be composed of distinct organs, corresponding to the various primitive colours, because the eye, when fatigued by long attention to one particular colour, is "flattered and relieved" by looking at an opposite one? The absurdity is no less when the same thing is affirmed of the mind.

Partial insanity furnishes our editor with what he seems to regard one of his triumphant arguments. "It is *impossible* (says he) to reconcile the facts of partial insanity, or monomania with the notions of the unity of the brain. Some persons are, we know, only deranged on one subject, and display, in other respects, a consistency of conduct, and consequently a strength of reasoning, that would deceive a stranger." We do not pretend to reconcile the occurrence of partial insanity "with the notions of the unity of the brain;" but we conceive that it is equally irreconcilable with the phrenological notions of a plurality of cerebral organs. We most assuredly gain nothing by ascribing the occurrence of partial insanity to a morbid condition of some one of these supposed organs of the brain. Each organ, according to this system, is not confined in the exercise of its function to a *single specific idea,* but to an extensive class of notions. Thus the organ of *ideality* has the exclusive command of every thing relating to fancy, enthusiasm, and poetical feeling. Now, should we be told that the man who believes himself a plant, though correct upon all other subjects, labours under a disease of the organ of ideality, how, we might ask, can this organ, which forms such an infinite variety of ideas, manifest derangement on one particular notion alone, while in every thing else its operations are correct? Upon this subject we are again tempted to quote, with due seriousness, from the learned Martinus Scriblerus; and we do this the more willingly, as we feel a desire to do justice to the reputation of that renowned philosopher, by showing that he was, in truth, the original contriver of this wonderful doctrine of our phrenologist. "It is well known, says this erudite genius, that the

brain is divided into a congeries of glands; that a gland is nothing but a canal of great length, variously intorted and wound up together. Simple ideas are produced by the motion of the animal spirits in one simple canal; and when two of these canals disembogue themselves into one, they make what we call a proposition; and when two of these propositional channels empty themselves into a third, they form a sylogism or a raciocination. Memory is performed in a distinct apartment of the brain, made up of vessels similar, and like situated to the ideal, propositional, and syllogistical vessels in the primary parts of the brain. After the same manner, it is easy to explain why some people *think so wrong and perversely*, which proceeds from *bad configuration of those glands* Some, for example, are born without the propositional or syllogistical canals; in others, that reason, ill, they are of unequal capacities; in dull fellows, of too great a length."

After displaying the foregoing "proofs" of a plurality of organs in the brain, the editor goes on to refute the assertion which is sometimes made concerning the tendency of the doctrines of phrenology to fatalism and materialism. We shall not stop here to inquire whether this allegation be just or unjust. If the doctrines in question be true, their apparent tendency in this respect, can be no good reason for refusing them our assent. If they be erroneous, they must fall, and with them, all their sinister influence.

To the stubborn objection against the phrenological system, that persons have been known in whom the greater part of the brain was destroyed without their having manifested any particular deficiency of mental power, our editor opposes a few feeble observations of a general character. He prudently considers such cases as among the "exceptions which are wisely ordained by the Deity to make us feel our dependence and littleness," and "which make us exclaim—not all is faulty—but we know not all. What," he asks, "should we say to the flippant youth who, on hearing his professor explain the structure of the human frame, and the different parts

of which it is composed, as *all* necessary to health and perfect life, starts up and exclaims: Sir, you must be wrong. I have seen persons enjoy health who have not had all these parts you speak of." Why, truly, we should regard the professor who would make such a declaration a very great simpleton! Does our phrenologist pretend to say, that *every* part of the body is necessary to health and perfect life? A man may lose an ear, an arm, a leg, or his eyes, and enjoy health and life notwithstanding. But the man without eyes could not see, and one deprived of a leg would limp. Were the professor to say that the eyes are made to see, and the youth to exclaim: "Sir, you are wrong; I have known persons without eyes who could see perfectly well;" would we not laugh at the youth's absurd declaration? But does our editor, in the present instance, differ from the "youth" who should affirm such an absurdity? We are told that the brain is composed of a number of independent organs, each performing a function *which no other part of the brain can perform;* and yet, when we refer to cases where some of these organs were totally destroyed by disease, while the functions which are ascribed to them were nevertheless regularly manifested, it is insinuated that the function may have been performed without the existence of its specific organ. And, to crown the whole, this manifest contradiction is to be reconciled by the truly Socratic exclamation, "All is not faulty—but we know not all!"

After reasoning, as we have seen, with such prodigious acumen and cogency, the editor looks down from the summit of his conclusions with mingled sentiments of compassion and anger, upon the humble dissenters from his favourite doctrine, and bids them to look abroad on the world for demonstration of the correctness of phrenology! "Conviction," he foretells, "must be the result of such conduct. Then will they regret their former levity, and regard with *pity* and *sorrow*, the feeble attempts of those who substitute vague conjecture for observation, and ridicule the harmonies of nature." *Quis temperet á lachrymis?*

But it is time to leave the editor's *Preliminary Essay*, and to turn our attention to the more substantial portion of this volume, by Mr. Combe. The only way, observes Mr. Combe, in which the principles of phrenology can be demonstrated, is, by observing mental manifestations, and comparing them with developments of the brain. Before we enter upon an examination of the principles of this doctrine, we must advert to two objections which have been urged against this mode of philosophizing, and which our author oppugns with considerable ingenuity, though not, we think, with success. In order to establish the adequateness of this mode of inquiry, it must be shown, as has been observed by others, that the actions of men are in general the correct exponants of their faculties and propensities; and that the "true development of the brain can be discovered by observing the shape of the head." In relation to the first point, Mr. C. observes, that the "vulgar" in all ages, and in all nations, have entertained the opinion, "that men in their actions generally manifest their true faculties," and that "the faculties are permanent and uniform natural powers." The "vulgar" he says, have always had a correct practical knowledge of the philosophy of Gall and Spursheim upon this point, while the metaphysicians have been erring for two thousand years." Although not disposed to give full credence to the maxim of Bacon, "quod vulgo videtur verum, falsum est," or to maintain with him, that, "non est judicium in vulgo," we nevertheless confess, that, in the present instance we lack confidence in "vulgar opinion." The deceitfulness of man is, at least, as proverbial as the direct relation between his actions and propensities. "*The heart is deceitful above all things;—who can know it?*" This was the sentiment held in the days of the prophets; and it does not appear that human nature has as yet undergone the least change. Who, indeed, that has observed mankind, and looked into his own bosom, has not had the painful conviction forced on him, that dissimulation seldom permits the real sentiments and motives of the heart to be seen?

> ————There are soft smiles and gentle words,
> And there are faces skilful to put on
> The look we trust in—and 'tis mockery all!

It must also be observed, that religion, education, and a thousand extrinsic circumstances, may evolve, excite or subdue propensities, and exercise a powerful influence over the conduct of men. Are there not, in fact, whole societies, who, by the influence of their peculiar tenets of religion, have so subdued some of the dispositions of the heart, as to lead one who should judge from their actions alone, to believe that they did not possess them? Would any one, judging from the actions of that exemplary sect, the Friends, suppose that they were furnished with the "organ of combativeness?" And yet, if such an organ exist, there is no reason why it ought not to be found as frequently with them, as among the members of other sects. It does not, therefore, appear to us so easy to ascertain propensities from observing actions, as the author, and, according to him, the "vulgar," seem to think.

Admitting, however, that the propensities may, for the most part, be ascertained by observing the conduct of men, and that the true development of the brain may be discovered from the figure of the head; there are still strong objections against the sufficiency of this mode of philosophizing, to the establishment of the phrenological doctrine on a satisfactory basis. Let it be granted that a correspondence between particular developments and dispositions frequently exist; does this justify the conclusion, that the latter are necessarily dependent on the former? There are other coincidences of disposition and external appearances recorded, as remarkable as those upon which the system of phrenology rests. In a late English journal, it is stated that "John Lomas, who was hung for the murder of Mr. Morrey, in 1812, had *light hair, light eyes*, and a remarkable fair complexion. M'Ghinnis, for the attempt to murder Mr. Birch, of Stockport, in 1819, had *light hair*, *light eyes*, and a fair complexion; Samuel Fallows, for the murder of Betty Shawcross, at Whaley, had *light hair*

and light eyes. The governors of several of the principal prisons in this country, (England) have remarked, that in all cases of murder, and particularly in those which have been attended with circumstances of cruelty, the perpetrators have invariably had *light blue eyes and light hair.*" If mere coincidence of external appearance and propensity is to be taken as a sufficient foundation for the erection of a doctrine in relation to this point, we see no good reason why we may not as well ascribe the propensity of murder to the influence of light eyes and light hair, as to that of a particular elevation of the cranium, situated above the ears.

The confidence which seems to be reposed by phrenologists, in the inferences they draw from the concomitance of particular mental powers and cranial configurations, does not accord with some concessions which they are forced to make on this head. "We must keep in mind," says Mackenzie, "that the functions of the brain are affected by what medical men call *temperament.* Should we meet with a person who appears to have the higher faculties well developed, but in whom they are not so conspicuous, *we may be certain that there is something in the general constitution that affects the organs,* and this may sometimes be conjoined with neglected education."—(277). If, as is here stated, *temperament,* or constitutional disposition, can convert a person furnished by nature with large organs of intelligence, into a mere simpleton; or form a genius—ex quovis ligno—out of one deficient in these organs, what should prevent us from accounting, in the same manner, for the possession of particular dispositions and mental powers, when we find them accidentally associated with particular bumps on the cranium? When we meet with a person remarkable for a particular disposition of mind, possessing a certain bump on his cranium, why may *we* not as justly conclude, that the peculiar disposition is the result of *constitutional temperament,* and not of the supposed organ indicated by the bump, as do the *phrenologists,* when they find a person who has the same propensity, without this cranial protuberance? We are by no means dis-

posed to admit that mere temperament can have so extensive an influence. We make these observations, only to show the doubtful character of the inferences which are drawn from comparisons instituted between mental qualities and cranial structure.

A faculty, as defined by Mr. Combe, is "a power or capacity, which has innate functions and activity." The mind, according to the phrenological system, is composed of thirty-three faculties, and these faculties are said to reside in distinct and specific portions of the brain, supposed to constitute their appropriate organs. It is, moreover, maintained that the activity and energy of a faculty is proportionate to the volume or development of its *organ*, or of the peculiar section of the brain in which it resides; and that each faculty forms, recals, and combines a specific class of ideas, in a manner altogether independent and distinct from the others. According to this system, indeed, each faculty "appears to be a distinct mind, endowed with its own constitution, relations and laws." To this hypothesis, Mr. Combe anticipates an objection, which seems to us of much more weight, than he appears to be willing to acknowledge. If, as it is imagined, the mind be composed of a number of independent and essentially distinct faculties, how are we to account for the unity of consciousness? Mr. Combe does not pretend to clear up this powerful objection against the doctrines he advocates. He appears to consider it quite sufficient to observe, that "Gall and Spursheim avow themselves utterly unable to give any satisfactory explanation of the difficulty how consciousness comes to be single, while the instruments of thought are numerous."

There is another difficulty which occurs to us, upon this point, and which is, we think, quite as insuperable as the preceding one. *Judgment*, in the proper sense of the term, is said to be an attribute of the *reflective* faculties alone—that is, of the faculties of *comparison*, *casuality*, *wit* and *imitation*. All judgments, founded on comparison, are, therefore, formed in the organ of *comparison*. Now, it would appear to be necessary, that before a faculty can be employed upon

an object, a perception or consciousness of the object itself, must exist in the faculty. For no judgment can be formed of the relation of things, if no ideas of the things themselves are previously obtained. When, therefore, we compare a cube with a parallelopipidon, for instance, the organ of comparison alone, say the phrenologists, judges of their analogy. But the analogy between two such bodies, considered as geometrical figures, exists in their *forms* and *number of sides.* Now, the ideas of *form* and *number*, we are told, are formed, and, as it were, incarcerated in the two distinct and independent organs of FORM and NUMBER, and hence the organ of *comparison*, which decides upon the analogies derived from these ideas, must do so without having the slightest knowledge or perception of the ideas themselves.

These, however, are not the only objections which may be urged, with force, against the phrenological doctrine of a plurality of independent, and essentially distinct organs of the mind. Let us suppose a person to be looking at a number of pillars, placed in regular order, each differing from the others in colour, shape, size and form. The united impression of these on the retina, must undergo an analysis when it arrives in the brain, and the impression of *order* carried to the organ of ORDER; that of *form*, to the organ of FORM; that of *colour*, to the organ of COLOUR; and that of *size*, to the organ of SIZE. In what manner, then, are these simple perceptions associated, so as to give individuality to the compound idea? Even admitting, for a moment, the existence of the phrenological organs, it is evident that they cannot be reconciled with the phenomena of mind, without admitting also the existence of a simple and general intellectual power, which presides over and takes cognizance of the specific acts of these organs, and gives to their operations the character of individuality.

With regard to the different faculties enumerated in this system, it does not appear to us difficult to show that some of them, at least, do not exist. The faculty of *tune*, for instance, when correctly viewed, is manifestly not to be explained on phrenological principles. It may, we think, be

laid down as an incontrovertible position, that every sensation derived from an external impression, necessarily depends for its character on the peculiar nature of the impression. As the impressions are varied, so are the sensations which are derived from them. When the string of an instrument is touched, an impression or excitement is produced in the organ of hearing; the impression is transmitted to the common sensorium, and there gives rise to the sensation of sound. If a different string be touched, the mind perceives a different sound, because a different impression is made on the auditory apparatus. Hence the character of a sound depends on the nature of the impression conveyed from the ear to the brain. But the nature of an impression must depend both on the nature of the exciting cause, and the peculiar organization of the part which receives the impression. When, therefore, a note, sounded in the presence of two individuals, is listened to with delight by the one, and indifference, or even repugnance by the other, the dissimilarity of the sensations can only depend on the dissimilarity of the impressions made on the ears of these individuals, and since the external cause is the same, the difference in the impressions must arise from the difference of the intimate organization of their respective organs of hearing. We have a right, therefore, to conclude, that a person who is incapable of perceiving harmony or melody of sounds, must be deficient in that peculiar organization of the auditory apparatus which is requisite for the reception of such impressions as are suited to give rise to the perception of harmony and melody.

Upon this subject, Bichat makes some very interesting observations. "The precision," says he, "of our sensations, appears to be so much the more perfect, as there happens to exist a more exact resemblance between the two impressions of which each is the assemblage. We see imperfectly, when one of the eyes, better constructed and stronger, is more vigorously affected, and transmits to the brain a stronger impression. It is to avoid this confusion that one eye is shut when the action of the other is artificially augmented by a convex

glass: the glass destroys the harmony of the two organs, and we use but one, that there may be no discordance." "What we have said of the eye may be applied also to the ear. If, in the two impressions which unite in giving the perception of sound, the one is received from an organ stronger and better developed than the other, the brain differently affected by each organ, will be the seat of an imperfect perception only. It is this which constitutes a false ear. Why is one man painfully affected by a dissonance which is not perceived by another? It is because in the former the two perceptions of the same sound are confounded in one; this is precise, and distinguishes the slightest fault in the song; while in the latter the two ears offer different sensations—the perception is habitually confused, and cannot estimate the defect of harmony." (Philosoph. Research. p. 16.)

The observations of Mr. Combe, in contravention to these objections, possess neither force nor ingenuity. "The auditory apparatus," he observes, "being excited to activity by an external cause, produces only the impression of sound; and here its functions terminate. If, besides, the faculty of tune is possessed by any individual, melody in sounds is perceived by that faculty. If the faculty is not possessed, such perceptions cannot exist. There is no proportion betwixt the perfection of hearing and the perfection of the power of perceiving melody. If it were part of the functions of the auditory apparatus to give the perception of melody, how does it happen, that in one individual, the apparatus can perform only one half of its functions, while in others, it performs the whole. Finally, hearing cannot produce music; because the auditory apparatus is excited only by sounds which are already produced." This language is as vague as the sentiments it conveys are unphilosophical. Sounds, we are informed, are produced before they reach the ear! This is a novel doctrine in philosophy, but nevertheless, in our opinion, a very absurd one. As well might we say that the pain of burning resides in the fire, and exists before the fire makes an impression on the body, as to affirm that sound exists before an excitement

is produced in the organs of hearing. The external *cause* of sound can bear no more resemblance to sound itself, than the instrument which wounds, bears to the sensation of pain it produces. Sound, philosophically viewed, is a *sensation*—a *perception* of an impression made on the ear by an appropriate exciting cause. It cannot exist without previous excitement and perception. Mr. C., in the above passage, seems to think, that those who oppugn the system of phrenology on the present point, are disposed to maintain the absurdity, that sound is *perceived* by the auditory apparatus." "The organ of hearing," he observes, "being excited by an external cause, produces only the *impression* of sound; and here its functions terminate!" And this he considers a good argument in support of the doctrine of a specific internal organ of *tune*. But admitting, what indeed we are fully disposed to do, that the external *cause* of sound produces only an *impression* on the auditory apparatus, are we to imagine that the *affection* of the mind produced by this impression, and which we call sound, is not determined by the peculiar character of the impression itself? The auditory apparatus has more to do than merely to receive the external impressions of the cause of sounds: it has, besides, an intimate influence on the character of sound itself.

The same observations apply with equal propriety to the organs of vision. It has already been said that the clearness and accuracy of visual perceptions depend essentially on the degree of perfection of the organs of sight, and especially on the harmony which subsists between the impressions made on the two organs of this sense. This being the case, the phrenological doctrine of *colouring* is obviously erroneous and hypothetical. The faculties of *form*, *size*, *weight*, and *order*, if they exist, manifestly depend for the degree of their power on the distinctness of the perceptions received through the organs of *sight* and *touch;* and they stand, therefore, in the same predicament with the faculties of *tune* and *colouring*. Mr. Combe observes, that the different faculties already mentioned, cannot belong to a single power of the

mind; since we find individuals who excel in the powers of *some* of these faculties, while they are deficient in those of the others. Thus, a person may be able to perceive and distinguish the nicest shades of colour, and yet be incapable of perceiving melody or harmony in sounds. A correct explanation of this fact may be found in what we have already said concerning the organs of hearing and sight; for as there can be no doubt that a person may be furnished with a delicately organized auditory apparatus, and at the same time possess less perfectly constituted organs of sight, it is manifest that the circumstance in question will occur as often as such a disparity exists in the relative perfection of these senses.

The faculty of *language* is said to be that faculty by which we acquire and use *arbitrary signs*. Thus, this faculty has the exclusive power of *perceiving*, *combining*, and *recalling* those signs or sounds, to which we attach particular notions, for the purpose of communicating these notions to others. Let us therefore suppose a person to be looking at a number of stenographic characters, to each of which a particular idea is attached. What are the mental perceptions of this person? If they be simply the *ideas* to which the characters are arbitrarily attached, why imagine a distinct faculty for their perception, when the same organ which perceives these ideas, at the presentation of their real objects, ought to be competent to recall them when suggested by an arbitrary sign.—If, on the contrary, the characters themselves constituting the arbitrary signs, be the sole objects of this supposed faculty of *language*, then we must admit that this faculty performs functions which belong to some of the other faculties; for as such characters, when divested of their associated notions, are nothing more than sensible objects, it is manifest that their perception is referrible to those faculties which, according to this system, are designed to take cognizance of such objects.

The faculty of *locality*, we are told, when powerful, "forms vivid and distinct conceptions of situations and

scenery which they have seen or heard described, and they have great power in recalling such conceptions. It gives the desire for travelling. It inspires the traveller and practical geographer, and is an ingredient in the endowment of a genius for landscape-painting. It gives the capacity of perceiving space, distance, and localities." This wonderful faculty is situated immediately above the internal angle of the eyes. Let us examine it more intimately. A tree may be considered as an individual from among the variety of objects which enter into the composition of a romantic scene. Suppose then a tree be viewed apart from those other objects with which it is associated in a natural scene; will it be said that the conception of the idea of the tree is formed in the organ of locality? We might as well assert that the idea of a horse, when we look at that animal, is formed in this imagined organ. If the individual objects, therefore, which compose a landscape, be not conceived by the faculty, can a plurality of them, however diversified in appearance, become the object of this faculty? But it will be said, that it is not a single tree, or any individual object entering into a beautiful prospect, concerning which the faculty of *locality* is employed; but that it is the total image, which arises from the contemplation of a variety of the different objects that unite to form a picturesque scene. The united impression of a group of individual objects, must, therefore, according to this opinion, be on *one* of the supposed intellectual organs, whilst that of the individuals themselves, separately considered, is made on *another;* a conclusion which does not speak much in favour of this visionary system. A tree, or a rock, or a rivulet, is to a landscape, what a *tone* is to a strain of harmony. The proper disposition of the former class of objects constitutes romantic scenery—whilst the proper mingling and succession of simple tones, produces the swell of harmonious music. But as, according to the phrenological system, it would be absurd to say, that the perception of simple *tones of melody* are formed in an organ distinct from that which gives the perception of harmony, so it would be equally absurd to af-

firm that the conception of the individual objects composing a landscape, and of the landscape itself, were formed in two distinct organs of the sensorium.

In the prenological system, the organs which are supposed to give the *propensities*, are specifically distinct from those which give the "knowing faculties." "The faculties of the *propensities* and *sentiments*," says Mr. Combe, "do not form ideas, and have not the attributes of perception, conception, memory, and imagination." The "knowing faculties," on the contrary, are said to possess these "attributes." How are we to reconcile this doctrine with the alleged powers of the organ of *locality?* This organ, we are told, takes cognizance of distance, space, and locality, and forms ideas of natural scenery. In this respect, therefore, it is a "knowing faculty." But it is also said to give *the propensity of travelling;* and in relation to its powers in this way, it is an organ of *propensity*. This doctrine assumes, too, that the desire for travelling is inspired solely by a superior capacity for perceiving, space, distance, and locality, and for forming vivid conceptions of natural scenery. It is scarcely necessary, however, to observe, that the motives which induce men to travel, are very often totally distinct from those which are derived from a fondness for natural scenery and localities. The desire to contemplate man under the diversified conditions in which he exists in different countries and states of society—to examine him as a moral and political agent—to trace the influence of climate, customs, education, and of civil and religious institutions on his character—these are among the most powerful and dignified incentives to travel.

We might go on to point out many other inconsistencies in relation to the supposed powers of the faculties enumerated in this system; but the limits which we have prescribed to ourselves for this review, do not admit of a more full exposition on this head.

The advocates of phrenology seem to place much reliance in the fact noticed by physiologists; that "the pre-eminence of man and the superior animals over others, in point of in-

tellect, is accompanied, as a general rule, by a proportionate size of that part of the head which contains the brain." As a *general rule*, this unquestionably obtains. But although it be true that the size of the brain is generally in a direct proportion to the capacity of the mind, no evidence is thereby furnished of the leading principles of the doctrines of Gall and Spursheim—of a multitude of independent and specific organs in the cerebral mass. This fact accords no better with the phrenological, than the metaphysical doctrine of the mind. We freely admit that the brain is the organ of the mind, and that the degree of mental capacity depends essentially on the degree of perfection of this organ. The charge of materialism does not, in justice, apply to this doctrine. The immaterial soul manifests itself through the medium of this organ. It is constantly influenced by the physical condition of the animal system. Its energies are repressed or excited by causes acting on the brain. If a temporary change in the condition of the brain is thus capable of producing corresponding changes in the activity of the mind, what reason is there to deny, that an original and permanent modification of the *organization* of this organ is capable of giving a corresponding modification to the intellectual faculties. This doctrine, in relation to the connexion between mind and its material organ, is, however, not peculiar to the phrenological system; nor do the fundamental principles of this system derive any support from it whatever.

It must not be supposed, however, that mere *magnitude*, or increased volume of the brain, is the cause of superior intellectual capacity. Were this the case, the size of the brain and the powers of the mind would necessarily *always* bear a direct relation to each other. In relation to the human species, this is far from being even generally found to obtain. We not unfrequently see men exceedingly stupid and insensible, and who are nevertheless furnished with prodigious heads; whilst, on the contrary, men of small heads are as often found to be possessed of a high degree of intellectual vigour. There must, therefore, be something else connected

with the organic condition of the brain, which constitutes the proximate cause of diversity in mental capacity; and as we reject the influence of mere quantity or size, we infer that the internal *organization*—the intimate and essential structure of the substance of the brain itself, constitutes the real cause of this phenomenon.

Observations drawn from comparative anatomy, have been much employed in support of phrenology. It does not appear, however, that the arguments derived from this source, are of much avail to this system. In many striking instances, indeed, they are in direct opposition to its principles. Dr. Warren states, in his recent work on the sensorial and nervous systems, that in the crania of the lion and large dogs, the external protuberances indicative of the organs of *combativeness*, are entirely wanting. He states, also, that the skulls of two *lions*, and of various large dogs in his possession, are narrower in that part where the organ of combativeness is supposed to be located, than in the skulls of various monkeys and the *sheep*. "The organ of amativeness," says Dr. Warren, "is placed in the cerebellum, its region externally corresponding with the lowest posterior part of the os occipitis. An examination of this region in animals remarkable for this propensity, does not exhibit a considerable development of the part. In the monkeys, generally, it is much less developed than in man; and in the baboon, the most extraordinary of all animals for the propensity, it is in no way remarkable." The organ of tune, also, he says, cannot be verified on the skulls of various birds. We have read what Dr. Coates, of this city, has published concerning these statements of Dr. Warren, and we intended to pay particular attention to his remarks, many of which are very ingenious, though, we think, not successful. As a proper discussion on this head, however, would extend this article beyond the limits which we have allotted to it, we are obliged to postpone our observations to some future occasion.

Phrenologists are constantly referring us to observation for proofs of the truth of their system. They allege that an

adept in their doctrine—a well disciplined skull-feeler, rarely fails in ascertaining the true character of those whose crania he explores. They are not easily discomfited. Should they find an organ fully developed without a corresponding strength in the faculty; they have several convenient subterfuges at hand:—they may refer it to general temperament—or to the predominance of some other organ. If a strong propensity is noticed in an individual, without a development of its supposed organ—why it is temperament too; or the influence of some other organ. The truth, however, is, that the coincidences upon which they rest their doctrine, are much less frequently found than they would have us to believe. It is stated in the late British journals, that the London phrenologists evinced great eagerness to examine the head of Thurtell, after his execution. Their hands were dexterously passed round, and above his ears, but the expected bump of *destructiveness* was not to be found—it is said that they were much dismayed, when, in addition to this deficiency, they found the organ of benevolence prodigiously developed. They were, however, somewhat relieved, by finding that he had prominent organs of *courage.*

As to the assertion, that the judgments formed of the dispositions and intellectual qualities of individuals from cranial configurations, are in general correct; we place no more confidence in them than we do in the testimonies which are so frequently adduced in proof of the phenomena of animal magnetism. It was at one time generally believed that the king of England had the extraordinary power of curing scrofula barely by touching the patient with his hand. Many of the most eminent medical writers of the seventeenth century, testify to this wonderful virtue in the royal touch. Wiseman, physician to Charles II., affirms, that he saw more than two hundred speedily and perfectly cured by the touch of Charles; and he vituperates those who would deny or doubt the truth of such cures. We now know, however, that the powers formerly ascribed to the royal person in this respect, have as little foundation, in truth, as the infallibility

of the Pope. Time, we apprehend, will also set men's wits to rights with regard to the speculations of phrenology.

E.

Art. VII.—*Surgical Anatomy of the Head and Neck*, illustrated by cases and engravings, by the late Mr. Allan Burns, of Glasgow. First American edition, with a life of the author, and additional cases and observations, by Granville Sharp Pattison, Surgeon, Professor of Surgery in the University of Maryland, &c. Baltimore, 1823. Octavo, pp. 512.

The first edition of this work was published in Glasgow, in the year 1811, a few months before the death of its author. Mr. Burns was, probably, one of the most distinguished teachers of anatomy and surgery who have lived in modern times. His demonstrations were so lucid and interesting, and the zeal with which he cultivated his science was so manifest, that his pupils were animated with an uncommon share of enthusiasm. We do not recollect to have heard any other teacher spoken of with so much warmth and admiration, as we have been impressed with by those who formerly attended his lectures in Scotland. It is not singular, therefore, that such a spirit of energy and originality should have been infused into his writings, as to force them upon the attention of the public. In a short time after its publication, scarcely a single copy of this work could be purchased in Great Britain; and the edition with which Professor Pattison has favoured us in this country, must now prove peculiarly acceptable to the profession.

The original text has been so long before the public, that an analytical examination of it will not be expected by our readers. We shall, therefore, devote our attention chiefly to the additions and commentaries of the American editor. We should do wrong, however, not to notice the peculiar merits of the work, as connecting relative anatomy with the history of operations and derangements of structure. In his chapter on diseases of the neck, for instance, he describes the connections of the fasciæ with the several muscles, glands and

blood-vessels; the mutual relations which occur between these organs themselves; and the diversified alterations of arrangement produced by the development of tumours. But this is not the only characteristic of the work that can prove useful to a practitioner. It contains, also, many interesting details of cases, and much enlightened pathology. To quote from recollection merely, we will describe the history which he relates of a young man, whose thymus gland having been previously enlarged by whooping-cough, was entirely discharged in form of a scrofulous abscess. Ever after, this patient was afflicted with great difficulty of inspiration, in consequence of the want of resistance to atmospheric pressure above the sternum, from destruction of the cervical fasciæ in that situation. This case was brought forward, not only to illustrate the importance of the fasciæ, but also to point out a method of remedying such an inconvenience, on future occasions, by the use of an adhesive plaster, coated over with an alcoholic solution of sealing-wax.

He also alludes to cases of enlarged conglobate glands, which were mistaken for diseases of subjacent or surrounding conglomerate ones. There can be no doubt that some of the alleged operations for extirpating the parotid and thyroid glands, must have been cases of this kind, wherein these organs were partially displaced, and their positions occupied by a morbid development of the lymphatic granules. We have seen patients with such tumours, respecting the nature of which it was very difficult to form a precise opinion; and we have witnessed one operation, in which a friend of ours removed a sub-cutaneous tumour, that counterfeited the appearance of an enlarged parotid. To guard against similar mistakes, the observation of our author should be recollected. The origin and progress of the disease should also be adverted to, at the same time that we form an exact estimate of its relative connections with surrounding parts.

He notices some cases of glandular swellings in the neck, which were mistaken for aneurisms of the great arteries in that region. Abscesses over the course of the blood-vessels,

are, also, sometimes confounded with aneurisms, in consequence of the pulsatory motions which are communicated to them. In order to distinguish between such dissimilar affections, it is only necessary to compress the tumour between the fingers, from side to side, when the peculiar thrilling sensation will not be perceived, except in cases of real aneurism. There are, however, several other circumstances connected with the history of individual patients, and the existing state of their constitutions, which may assist the discriminating practitioner in forming his diagnosis.

Mr. Burns suggested an ingenious method of extirpating tumours, when they are intimately connected with important parts. It consists in laying aside the scalpel, immediately after their surface has been exposed, and completing the operation by the fingers, or some blunt-pointed instrument. The morbid parts are thus torn away from their attachments to surrounding structures, with, comparatively, no danger of hæmorrhage.

"After the tumour has been in this way removed, it is to be carefully washed and examined, lest any of the morbid substance has been torn, and left behind. Even when we are satisfied that the tumour has been removed entire, the finger is to be run over the wound, to ascertain that no enlarged gland, or indurated cellular membrane, be left. But here it is proper to remark, that the ruptured arteries, which are felt like small hard points projecting from the wound, are not to be confounded with specks of diseased matter." In discussing the subject of sub-cutaneous tumours, he also declares, that "where the tumour is small, it is preferable, when the whole of it is uncovered, to grasp it firmly between the fingers, and suddenly, with a twisting motion, to wrench it from its place." He describes a tumour over the parotid gland, which, "when not larger than a walnut, by its pressure, produced absorption of a part of the parotid, by which it made a bed for itself in the substance of the gland. In such a case, it is evident that it would be very difficult to dissect away the tumour with the

knife, without, at the same time, injuring the parotid, which is avoided by tearing away the tumour with the fingers."

Our readers, however, should not be misled by these observations, to suppose that tumours can generally be extirpated without a free use of the knife. We have often found it to happen, that the investing cellular tissue has been condensed into such firm bands and membranes, as would defy all efforts to produce laceration. Nevertheless, we can easily conceive that the plan proposed by Mr. Burns may prove, in some cases, highly advantageous to the operator, as well as to the patient.

The ideas of the late celebrated John Bell, respecting the origin of specific tumours, are ingeniously controverted in this work. "Mr. John Bell maintains that, originally, every tumour is produced by an excess of healthy nutrition; and he would persuade us, that ultimately the parts are modified in form and character, by many changes produced by occasional inflammation and ulceration." "One would hardly have expected such an assertion from a surgeon who would make us believe that he had traced the nature and properties of these morbid productions from their simple beginnings to their final termination. Shall Mr. Bell convince any one that in cancer or scrofula the tumour was, in the first instance, formed of healthy parts; but that, eventually, the swelling obtained the peculiar character belonging to these different affections, by occasional inflammation or ulceration? I should be sorry to waste time in animadverting on this conjecture, were it not that to me it appears a point highly requisite to be well ascertained. It must be the regulator of our practice. If, primarily, every tumour be simple, then the whole art of the surgeon must consist in keeping it simple. His object must be to avoid the induction of occasional inflammation or ulceration. There is, in this hypothesis, nothing precise, and nothing really useful; but, if admitted, there is much positively hurtful, as it will lead to a timid and procrastinating practice." "As, in the present state of our knowledge, we have it not in our power to do more than form a rude notion of the nature

of swellings, we often mistake a specific for a simple tumour, and waste time in useless endeavours to promote its removal without an operation. We, in fact, allow it to acquire such a size, and to form such connexions, that when we become convinced of its intractable disposition, it is no longer optional with us to take up the knife with any reasonable prospect of success."

The good sense displayed in these observations, must be appreciated by every one who has had the misfortune to be consulted in the latter stage of specific tumours. In consequence of unwarrantable procrastination on the part of the patient, or his medical advisers, the neighbouring parts have then generally acquired a disposition to morbid action; which rarely fails eventually to reproduce the disease, however perfectly an operation may have been executed. The constitution, also, becomes so much contaminated, as frequently to develop the same disease in remote parts. Such deplorable circumstances can only be prevented by the timely performance of an operation for removing the whole morbid structure. Delay can never be attended with advantage, while the disease is, at the same time, progressing, and complicating itself with greater difficulties.

A remarkable case of emphysema is detailed on pages 81—2. A firm, tense tumour, of the size of a man's fist, had existed five years after the reception of a blow on the lower part of the neck, and behind the sterno-mastoid muscle. "During the sixth year, it received a very rapid addition to its bulk. At this time it measured six inches in diameter, seemed to be confined by a firm and dense covering, and the morbid parts had an obscure fluctuation. From first to last the tumour had been productive of very little pain. Judging, from the apparent fluctuation, that the tumour was encysted, it was resolved at a consultation to puncture the swelling, draw off its contents, and then pass a seton through it. By plunging a lancet into it, only a very small quantity of blood, partly coagulated and partly fluid, was discharged,—a quantity so trifling, that after its evacuation the size of the tumour

was not perceptibly reduced. A seton was passed through the swelling. At this time the man was in perfect health. About ten hours after the operation the patient was seized with extremely violent rigors, followed by heat, thirst, pain in the back, excessive pain in the tumour, and oppressive sickness. To remove the irritation, the seton was withdrawn, and a cathartic exhibited. The pain in the tumour, however, and the general uneasiness continued to increase; and thirty hours after making the puncture, *air began to issue from the tract of the seton;* and afterwards the cellular membrane of the neck, and of other parts of the body in succession, became distended with a gaseous fluid. In the course of a few hours more, the man died. Twelve hours after death, when the body was free from putrefaction, it was inspected. The emphysema was neither increased nor diminished since death, and some idea may be formed of its extent when the scrotum was distended to the size of the head of an adult. Even the cavities of the heart, and the canals of the blood-vessels contained a considerable quantity of air. We could discover no direct communication between the tumour and the trachea or lungs, although such was carefully sought for." In commenting upon this case, our author insinuates that the emphysema could not have resulted from the puncture of the tumour, or from the introduction of the seton. He ascribes it to an accidental rupture of some of the bronchial cells, and attempts to explain it by detailing another case, in which the disease originated from mere coughing. He supposes that, in such instances, the air is first forced into the cellular tissue which connects the bronchial vessels to each other; from whence it travels through the root of the lung, into the mediastinum, upwards to the neck, and to all parts of the body.

But, however ingenious this explanation may be, it does not appear sufficiently clear to outshine, in our minds, the manifest connection between the wounds inflicted above the clavicle and the subsequent development of emphysema. The pleura, as we have stated on a former occasion, and in another place, extends considerably above the superior limits

generally assigned to the cavity of the chest. It lines the muscles and vessels of the lower part of the neck, in such a manner as to be exposed to the effects of injuries inflicted upon that region. The lung, on each side, also, extends upwards, in close contact with the pleura, so as to form a considerable projection above the level of the first rib. The tumour, in the case just described, does not appear to have projected much externally, although it covered a large surface of the inferior region of the neck. It was flattened, and bound down firmly by the superincumbent fasciæ, so that it probably lay in contact with the superior limits of the corresponding pleura. Neither the puncture, therefore, nor the seton, need have been carried deeply below the integuments to have penetrated through the pleura, into a portion of the lung, so far as to give origin to the emphysema. This explanation is not obviated by the fact that Mr. Burns did not discover any connection between the tumour and the lung of the same side. It could hardly have been expected, indeed, that a small puncture should have been traced into the chest when it was not looked for, and when the surgeon himself was not aware of the extension of the pleura above the level of the first rib. The ideas which we have advanced are corroborated by the fact that excessive pain in the back, and high febrile irritation, exhibiting the characteristics of thoracic inflammation, soon followed the incisions into the tumour. It should be recollected, moreover, that the emphysema first appeared in the form of air rushing out through the wounds.

But we will not undertake to deny the applicability of Mr. Burns' hypothesis to the explanation of some forms of emphysema. This disease has been known to result from active exercise, as well as from coughing. Wilmer saw it occur from a tedious labour; and several cases are recorded in which it took place during the progress of constitutional diseases. In such instances, no wound or external injury could have been concerned in the production of emphysema; and it is probable that the suggestion of our author may be applicable to them. Still, however, it should not be forgotten, that this

disease may be produced by a direct secretion of air into the cells of the mucous web in various parts of the body.

At page 380, an interesting examination of the connection which obtains between the two optic nerves, is detailed. A female, forty-one years of age, was affected with fungus hematodes of the left eye, which eventually destroyed her life. The interior of the ball was found to be filled with a dark, inky substance, which probably originated from a degeneration of the pigmentum nigrum. The optic nerve was of its natural size, but of a black colour where it entered the foramen opticum. "Within the cranium it was as thick as the little finger, and as dark in colour as that part of it in the orbit. The junction of the nerves was so much enlarged, that it formed a tumour extending into the third ventricle. As from the dark colour of the diseased parts, this was a favourable opportunity for ascertaining whether the optic nerves decussate each other, or merely come in contact, I examined carefully the state of these parts. I found the dark colour extending much beyond the point where the nerves join; but this change of colour was *confined to the left side, or to the nerve of the affected eye.* On the right side the nerve was of its natural size and colour, and was merely attached to the black diseased parts by cellular threads. This dissection, therefore, clearly proved that the nerves did not, in this individual, cross each other."

This case, however, by no means decides the anatomical question with which it is connected. Meckel, in his edition of Voigtel's Pathol. Anat. Vol. I. p. 678, quotes Sœmmering, Blumenbach, Billman, Ackerman, Michælis, and Metzger, for instances of an opposite character, in which the nerve of a diseased eye became, on the other side of the decussation, diseased, smaller, and discoloured. The same author also mentions, under the head of *abnormal forms,* some cases in which the decussation of the optic nerves not only did not take place, but in which they went to the brain separate from each other. He quotes Vesalius, Fabricius, Morgagni, and Prochaska, for cases of this kind. From

such discordant facts it is impossible to draw any positive conclusion respecting the natural mode of connexion which subsists between these organs.

The short extracts we have now made from the voluminous work before us, will be sufficient to convey to our readers some idea of the nature of the whole performance. It abounds with practical observations, and is characterised throughout by good sense and sound knowledge. Although the style is not always classical, nevertheless it is explicit and instructive. We are convinced, therefore, that the book deserves a much more extensive circulation among the members of the profession in this country, than it has heretofore received.

The commentaries of the American editor are not so numerous as, from the character of those with which he has favoured us, we could have wished. He objects to the operation proposed by Mr. Burns, for removing an enlarged thymus gland, on the ground that the mesenteric glands are always diseased, and that scrofula is at the same time invariably manifested in other parts of the system. "If this opinion be correct, the operation recommended for the removal of the thymus gland, although it might relieve the dyspnœa, could, in no case, restore the patient to health; the diseased enlargement of the mesenteric glands remaining, would speedily destroy life." For our parts, however, we cannot altogether realize the force of this objection. The mesenteric disease is not always incurable, nor does scrofula, even in its most inveterate forms, prove, in every instance, irremediable. Provided, therefore, the injurious effects of an enlarged thymus upon respiration and deglutition be removed, the subsequent treatment of the constitutional disease will become effective in cases that otherwise could not have been brought to a successful termination. A much more serious objection to such an operation may be anticipated from the dangers that would immediately result, in consequence of an exposure of the cavity of the mediastinum, and of the great vessels which are contained within it.

A judicious commentary is given, under pages 54—5, on the

subject of enlargement of the lymphatic glands, in the vicinity of specific tumours. Mr. Burns gave it as his opinion, that carcinoma is never attended with a contamination of the neighbouring glands, however much enlarged they may be from sympathetic irritation, until the ulcerative stage has commenced. In scirrhus of the breast for instance, accompanied with enlargement of the axillary glands, "these tumours occasionally disappear after the removal of the mamma, which clearly shows that they were not dependent on specific contamination." The editor expresses his conviction of the opposite opinion in decided terms. "In many instances where the most accurate and minute dissection has been unable to detect, in the original tumours, either incipient ulceration or fungus, the structures of those which have arisen secondarily, have been found to present the same characters of their being of a specific nature, as those exhibited in the primary affections." The rule of practice which his experience would incline him to inculcate, therefore, would be to remove all the enlarged glands which derive their lymphatics from the diseased part, at the same time that we extirpate the primary tumour.

Under page 115, he also very properly takes exception to the judgment pronounced by the author against operating upon tumours which are intimately connected with the great arteries. "Mr. Burns' observations, as to the impropriety of attempting the removal of a tumour when we have ascertained that it is connected with the carotid artery, go a great deal too far. The connexion of an artery with the tumour, or even the vessel being surrounded by its substance, should not offer to the dexterous surgeon an insurmountable objection for (to) its extirpation, when he is satisfied that this is required for the safety of the patient."

The editor has made several other comments upon the original text, which it is unnecessary to notice in this place, although they may serve to elucidate the subjects with which they are severally connected. In an appendix, of seventy-five pages in length, he has given much additional matter upon

various points of the surgery of the head and neck, the substance of which we will attempt to communicate in a condensed shape.

The first six pages of the appendix are occupied by the detail of a very interesting case, in which severe pains in the lower part of the neck were treated, by several physicians in Glasgow, for a rheumatism. No relief, however, was experienced by any mode of treatment; and, at the end of six months, the patient died apoplectic. On dissection, it was found, by our editor, that the symptoms had been produced by an aneurismal tumour within the cavity of the chest. "There arose from above the arch of the aorta, a large tumour, which projecting externally, adhered firmly to the spinal aspect of the sternum. Upon separating this connexion, we discovered that the tumour was formed by an aneurism of the arteria innominata, and that the sternum, where pressed on, had become carious." The transverse vein was found to be compressed very firmly between the tumour and the sternum; its coats were contracted into a ligamentous cord, and its cavity was completely obliterated by coagulable lymph. The apoplexy is accounted for by these circumstances; but it is difficult to conceive how the whole progress of the disease could have been mistaken for a rheumatism.

The second note in the appendix, twenty-two pages in length, is devoted to the history of Dr. Mott's case, in which a ligature was cast around the arteria innominata, in the month of May, 1818. This operation was commenced with a view of securing the subclavian on the inside of the scalenus anticus muscle. On examination, however, it was found to be unsafe to apply the ligature in that situation; and the dissection was therefore continued towards the top of the sternum, until the greater part of the innominata was freely exposed. The ligature was applied just below the bifurcation of this artery, and the wounds carefully brought together by interrupted sutures and adhesive strips. On the twenty-sixth

day after the operation, the patient died of secondary hæmorrhage.

Respecting this case, Mr. Pattison has made the following appropriate observations: "Although this case terminated unfortunately, still its result, in so far as it concerns the operation of tying the arteria innominata, must be considered as conclusive evidence of its practicability. The operation was, in fact, so far as it was immediately concerned, successful; no alarming symptoms followed the tightening of the ligature, and the obstruction of the circulation through the channel of the vessel; the patient continued to improve in his health until the twenty-third day, when a hæmorrhage took place from an ulceration of the coats of the artery, an ulceration which has frequently occurred in instances where even the smaller arteries have been tied. If I might be permitted to offer a criticism on the performance of an operation so novel in its character, and so bold in its design, I should feel disposed to object to the previous exposure of the subclavian artery. Indeed, I am inclined to think that it was from the destruction of the vasa vasorum of so large a portion of the artery, that the fatal hæmorrhage is to be attributed; and I can hardly doubt, from the facts of the case, that had the arteria innominata been at once fairly exposed and tied, without any reference to the subclavian artery, the event would have been different. This opinion I deliver, not with the view of detracting from the credit due to the intrepid operator, but only from a desire to support and corroborate the sentiments of Mr. Burns."

It should be stated in this place, that Mr. Burns' was the first who proposed to secure the innominata by a ligature in a living subject; and that he was convinced of the practicability of this operation by repeated demonstrations of anastomosing routes, through which the circulation might afterwards be conveyed to the right arm, as well as to the right side of the neck and head.

The third note is taken up with a consideration of the difficulties which attend the operation for securing the carotid

arteries. Mr. Burns followed Sir Astley Cooper in his description of the rising and falling of the jugular vein, by which the operation was thought to be rendered exceedingly difficult. The editor comments rather severely upon this representation, which he declares to be altogether exaggerated, and calculated to create unnecessary alarm in the mind of a surgeon. He details two cases in which he performed the operation for securing the common carotid. In neither of these was there experienced any difficulty from the situation of the jugular vein. The fear of encountering it, however, produced much inconvenience in the first operation, which is very candidly explained by the editor.

In the fourth note, Dr. Physick's method of applying the wire to an enlarged tonsil is detailed. It differs from the common method, simply, in being left a much shorter time within the mouth of the patient. Formerly it was considered necessary to maintain the strangulation from two to four days, in order to effect a complete destruction of the tumour. Dr. Physick has been accustomed to use the instrument only twenty-four hours at a time; and he even proposes to devote no more than eight or ten hours to its application in each case hereafter. Although such a plan will probably succeed in all those cases of enlarged tonsils which are attached to the sides of the throat by narrow necks or pedicels, still we have our doubts whether it will not fail in cases of an opposite description. We once applied the wire very firmly around a large necked tonsil, and tightened it still more closely six hours afterwards. It was removed on the ensuing day; and although the whole substance of the gland appeared to be dead, only its exterior surface came away by the process of sloughing. The interior mass was afterwards left projecting into the throat, in the form of a raw and very irritable tumour, which required the knife for its removal. Whenever we again undertake to remove so large a tumour by the same means, we shall certainly leave the wire around it for the space of two or three days, tightening it repeatedly, at intervals of five or six hours each, until we are certain of the

complete strangulation of the whole mass. In all cases, however, where the parts are not too vascular, we would prefer the use of the knife, or scissors, for this operation. The danger of the hæmorrhage is altogether too much exaggerated by most authors. Neither the actual cautery, nor the use of styptics, will be required in ordinary cases of indolent tumours; and it is only in such that a judicious surgeon would think of operating by the knife.

The fifth note in the appendix is extended into an elaborate discussion of the methods of curing *aneurisms by anastomosis.* John Bell, the first author who clearly described this disease, considered it impossible to eradicate it except by extirpating the whole of the diseased part. Mr. Burns made what he considered a great improvement upon this idea, by ascertaining that after a portion of the tumour only has been extirpated, *the remainder may be destroyed by the steady application of pressure.* He relates a remarkable case which occurred to his brother John Burns, and himself, in which an operation was ineffectually performed for the purpose of removing an anastomosing aneurism from the temple. It was found that the tumour extended below the zygoma into the deep space behind the molar bone, from which situation it proved impossible to excise it. The hæmorrhage was very severe, and could not be suppressed by the use of ligatures. Pieces of sponge were then forced into the hollow above the zygomatic arch, and bound down by compresses and a roller. The consequences were, that not only the hæmorrhage ceased, but an entire absorption of the remaining portion of the tumour took place. The wound speedily granulated and healed, and no return of the disease was experienced.

In the time of Mr. Burns, it was considered impossible to cure this disease by any other method than those to which we have alluded. Mr. Travers, however, conceived a different opinion. In a case of anastomosing aneurism of one of the orbits, he tied the common carotid of the same side, and effected a perfect cure. This plan of curing the disease, by the indirect

operation of diverting the current of blood into other channels, was afterwards followed with success, by Mr. Dalrymple, in a similar case. Our editor has also adopted this method, in a case which he supposed to be situated in the branches of the internal maxillary artery. It occupied the walls and cavity of the antrum, and projected externally, as well as into the mouth, in the form of a monstrous tumour. It had been twice mistaken, and operated upon, for a polypus in the nose and antrum. These operations were attended with dangerous hæmorrhages, which compelled the surgeon to desist in each attempt. Before the patient applied to Professor Pattison he was nearly exhausted, by frequently repeated discharges of blood which occurred spontaneously on the slightest exertion. The carotid was taken up in this case, and a complete cure followed.

In the sixth note, the subject of fungous tumours in the antrum is considered. The common method of attempting to extirpate these tumours by the knife and actual cautery, after laying open the cavity in which they are contained, is not only exceedingly horrible and painful, but is seldom attended with success. The morbid parts can rarely be completely extirpated, and the disease is, in consequence, almost certain to return. The professor has proposed to treat such cases on, what he considers, a new principle—that of destroying their vitality by checking the circulation of blood through them. He recommends to tie the common carotid of the same side, and then to leave the parts to undergo the process of exhausting their enfeebled powers. We are well aware that the vitality, or capacity of a part for action, is proportioned to its vascularity. We know, also, that if we excite a part to the performance of an action beyond its power, that, as a necessary consequence, its vitality is destroyed; it sloughs, and is thrown off. Now, it happens that fungous tumours of the antrum generally are, like polypi of the nostrils, remarkably destitute of blood-vessels. The actions of a part, to restore its circulation, after the usual route has been suspended, are also greatly increased above those which ordinarily go on

in the same structure. Both of these circumstances conspire to render the ideas of the editor particularly applicable to the subject under consideration. Three operations have recently been performed for the cure of this disease, in the way thus recommended. The first was executed by Dr. Jameson, under an idea that previously securing the carotid, would more easily enable him to extirpate a huge tumour from the upper jaw. The professor, however, thinks that "the tumour of the gums," in this case, was nothing more than the extension of a fungus in that direction from the cavity of the antrum, the whole mass of which was subsequently destroyed, in consequence of obstructing the circulation through the parts by the ligature. The second case occurred to Professor Davidge, and is communicated at full length. The third operation of this kind was executed by Professor Hall of the same university. It is interesting to know that all three of these cases, so far as the tumours were concerned, terminated successfully.

The last note in the appendix contains the history of two operations on the lower jaw. The first of these was performed by Dr. Mott, for removing one side of the body and ascending ramus of the bone, after securing the carotid artery. The second was performed by one of the editors of this journal, for removing the body of the bone from angle to angle, on both sides, without interfering with the carotid. An operation, very similar to the first mentioned one, has been performed in Germany, by Professor Graefe, as will be seen in another part of this journal. The second case alluded to in the appendix, will be detailed at full length in the original department. We think it right, however, to state here, that Mr. Pattison was misinformed respecting the age, as well as of some other circumstances of the patient. She was six years old, instead of four, as is stated in the note.

In conclusion, we will not omit to state that the editor has promised to continue the work of Mr. Burns, in the form of a periodical volume, every autumn, until the whole surgical anatomy of the body shall have been considered and illustrated. We shall look for this original performance of Pro-

fessor Pattison, as no inconsiderable addition to our stock of professional knowledge. M.

ART. VIII.—*The Medical Recorder of Medicine* and Surgery.* Conducted by SAMUEL COLHOUN, M. D. For April, 1824.

Art. 1.—*An Abstract and Analysis of those causes, which favour and impede the progress of Medicine. By the Editor.*

WE ascertain from the title of this paper, that it is intended as an exposition of "those causes which favour and impede the progress of medicine," and we cannot but express our obligations to the author for this seasonable information; for without it, we should have been much puzzled to form a satisfactory conjecture, as to the object of this incomprehensible piece of rhetoric and ratiocination. As the whole of this production is altogether too recondite for our comprehension, we cannot pretend to give an analysis of its contents. We own that an author is not in duty bound to furnish brains to his readers, although, in the present instance, such an office would have been peculiarly useful and charitable. The initial paragraph runs thus:

"The early periods of Europe were obscured by ignorance, and harassed with the endless hosts of evils, which arise out of the unreclaimed state of the physical and moral world. The governments of that distinguished assemblage of communities, struggled long amidst these disorders, and at length *escaped beneath a brighter horizon.* Shedding their influence upon the peaceful occupations of agriculture, manufactures, and commerce, they mitigated the horrors of war, and evolved from the resources of the globe, the physical sciences, those vast engines which threaten, before many years *(shall)* have rolled away, to place within the grasp of power the most distant nations of the earth."

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* Formerly the title of this journal was "The American Medical Recorder *of Original Papers* in Medicine," &c. The present Editor has *improved* it to "The *Medical* Recorder of *Medicine,*" &c.

This splendid passage requires some elucidation. It must be observed, then, that after the governments of Europe had struggled a long time amidst the evils which arise out of the unreclaimed state of the physical and moral world, "they *escaped beneath* a brighter horizon." Having escaped beneath a brighter horizon, they shed their influence upon agriculture, manufactures, and commerce; and (being still under the bright horizon) they evolved the physical sciences "from the resources of the globe." We would willingly go on to explain the remainder of this paragraph, but we have, in vain, done our utmost to extract its meaning. No doubt there is some profound idea wrapped up in it, and we entertain the hope that it may yet be discovered and brought to light, by some more fortunate investigator. For our own parts, we would rather dig for an Egyptian mummy, or set about decyphering the hieroglyphics of a Babylonic brick, than make another attempt to this purpose.

We are indebted to the author for the following curious information. "*In the cultivation of science the mind is the great instrument,* and the mode of its application is simple." We do not regard this exactly in the light of a discovery; for the ancients appear to have had some vague notions of the instrumentality of the mind, in the cultivation of science, and there are not wanting obvious indications of a similar notion among modern philosophers.

The phraseology of the following passage is not much to our taste. "Every disease has a natural course of rise, progress and decline, and the powers of the system frequently conduct the patient to health, without interference of medicine. The remedy and nature, then, are two conflicting powers, *each* of which *may both* conspire to the same end, the extinction or relief of the patient, leaving the decision of the case" *(of what case?)* "a matter of great uncertainty, more particularly from the practice so prevalent in civilized communities, where observation is most accurate, and most to be trusted, of employing the aid of medicine in every case." *Each* of

which may *both* conspire! We must leave the sentiment entangled in this maze of words, to be unravelled by the ingenious reader at his leisure; for we do not deny that there is some meaning in it, though we confess, that we have not, as yet, been able to obtain a distinct view of it.

"The *same* causes" says our author "under the *same* circumstances, will have different effects." This is impossible.—Wherever the *same* cause produces *different* effects, the circumstances under which it acts or exists *must* be different. This is self-evident, and is universally admitted as such. We have picked up at random these specimens of our author's writing and philosophy. *Ex his disce cætera.*

Art. 2.—*An Essay on Strictures of the Urethra. By H. G. Jameson, M D. of Baltimore.*

This paper occupies thirty-three pages of the Medical Recorder, and is evidently put forth with no inconsiderable pretensions to novelty and experience. The author claims for himself the credit of having discovered a new cause for retention of urine in stricture, together with a peculiar mode of operating for its relief. After having introduced a sound into the bladder of a calculous patient, he once found that the instrument was repeatedly thrown backwards and forwards by "involuntary and very painful contractions of the muscles about the urethra." "The fact which I have just noticed, was the first link in the chain of circumstances, which led to the adoption of my present views of stricture. Although the state of things here in view did not lead to stricture, still it excited in my mind the query, whether so powerful an action of muscles, acting at right angles with the course of the urethra, might not subject the urethra to a species of strangulation. This supposition was greatly strengthened by the next case which I have recorded, and has been fully confirmed, at least to my own satisfaction, by my subsequent experience." It only surprises us that Dr. Jameson could

have supposed this to be a novel opinion, which required any confirmation from his own experience. Every surgeon who has been in the habit of passing a catheter into the bladder, must have met with such temporary obstructions in the membranous portion of the urethra, as can only arise from spasmodic contraction of the surrounding muscles. The same circumstance is noticed in almost every work which has treated on the subject; and the violent succussion sometimes communicated to the instrument, is also familiar to most practitioners.

The operation, however, which Dr. Jameson has proposed, to relieve stricture of the urethra, when complicated with such spasmodic contractions of the muscles, is not of so trite a character. It consists in cutting down to the bulb of the corpus spongiosum urethræ; and, after laying open the canal at that place, and passing a director into the bladder, in cutting boldly into the *muscles on each side*, by running a bistoury on both sides, along the groove of the director. He relates fourteen cases, in several of which he operated in this manner with success. We will quote his own words for an account of the result of his experience. "I have reported ten cases in which I opened the urethra, and thereby cured the most deplorable strictures; and one case attended with a relapse, in which mortification took place, and yet the patient recovered; making, of course, eleven successful cases. Among these cases, there have been four of mortification of the scrotum, one accompanied with fistula in the perineum, two in which the urethra was opened both through the penis and the perineum. And it is further to be understood, that no unfortunate cases have been concealed, and that from the time I commenced my operations upon the urethra, I have not lost a single patient."

Against the propriety or consequences of such operations, we have nothing to urge. But we are not perfectly satisfied with the explanation which Dr. Jameson has offered respecting the kind of advantages to be derived from them. He supposes that they prove useful merely by obviating the con-

tractions of the surrounding muscles; in effect, he ranks them in the same class with operations for the relief of strangulated herniæ. "I passed a straight blunt-pointed bistoury along the groove of the director, till I was satisfied it had entered the bladder, then an incision, about a line in length, was made on the left side of the urethra; withdrawing the bistoury and reversing the director, the bistoury was again passed in, and an incision to the same extent made on the right side. By these transverse incisions I intended to *cut the sphincter muscles* which surround the upper part of the urethra, and more especially, I wished to divide some fibres of the levator ani muscle, which arising from the pubis, pass down to be connected with the rectum, and in their course embrace the urethra, so as, under certain circumstances, to produce *strangulation of that tube.*"

We apprehend, however, that it will require an incision more than one line in breadth, on each side of the cavity of the urethra, to divide the muscles to an extent sufficient to produce the effects imagined by our author. But we do not feel disposed to insist upon this point, inasmuch as we do not conceive that the favourable result of these operations depended on such an effect. Although the muscles are capable of compressing the urethra, while in a state of active contraction, they never permanently exercise the same influence. If the symptoms of stricture continue for a great length of time, they must depend on some actual disease in the parietes of the canal itself, which alone will require to be acted on by whatever treatment we may institute.

There is great reason to believe that the spasmodic irritation which accompanies many cases of stricture, is improperly treated by some practitioners. Without regarding the general state of the system, or the condition of the affected parts, they immediately resort to various kinds of catheters and bougies, by means of which the irritation is always increased, and the lining membrane of the canal more or less lacerated. Frequently, false passages are made by forcible attempts to

surmount obstructions, under such circumstances: and, finally, the practitioner either abandons the case as hopeless, or resorts to some formidable operation for the purpose of eradicating the disease. Now, according to our experience, no such difficulties need attend the management of these cases. The symptoms of irritation should always be first subdued, by general and local blood-letting, the warm bath, fomentations, opium, &c., and no attempts at dilatation should ever be made until the spasmodic contractions of the muscles have, by these measures, been allayed. We do not speak of these circumstances, however, with any particular reference to the cases reported by Dr. Jameson. His character as a practitioner is altogether too respectable to allow us to suppose that he could have neglected such necessary preliminaries in the management of his patients. Still, we desire to be so fully understood, in relation to this subject, that our readers may not be misled to suppose that the operations described by him will be frequently called for in cases of stricture. We have been much employed in this kind of practice, and in only one solitary instance have we failed to conquer the most obstinate contractions of the urethra, by a careful use of bougies and catheters, aided by appropriate remedies. In the instance alluded to, a false passage had previously been formed behind the bulb, which rendered it impossible to convey a catheter, or bougie of any kind, towards the bladder. All such cases must, of course, require operations, like those described by Dr. Jameson, to afford complete relief. We agree with him, most fully, in the conclusion, that the practice pursued by him is altogether preferable to any mode of forming an *artificial* passage into the cavity of the bladder. But we cannot conceive that it will require so extensive a division of the surrounding muscles, as he imagines, in order to produce a complete relief from the symptoms of stricture. Convey a common sized catheter but once through the whole extent of the urethra, and there can be no difficulty in maintaining it there until the perviousness of the canal be fully ensured.

Art. 3.—*An Essay on the Autumnal Fever of Brunswick, Virginia. By Thomas B. Merrett, M. D.*

The author describes the symptoms of the autumnal bilious fever, as it appeared in the circle of his practice, clearly and methodically. His treatment is neither ultra-depletory, nor ultra-excitant. He divides bilious fever into simple, inflammatory, and congestive. In the inflammatory variety he found bleeding unequivocally advantageous. "I bled" says he, "in every exacerbation of the disease, after the chill, when I was present, if the arterial action required reduction, which was indicated by a tense or chorded pulse. The good effects of the lancet, when used so as to make an impression on the pulse, were often sudden and astonishing."

He found mercury injurious, when given so as to produce a general impression, before the declension of the febrile symptoms. In cases where topical disease had taken place, a slight mercurial impression became necessary towards the close of the fever.

Art. 4.—*An Essay on the Pathological Characters and Sanability of Consumption.—Extracted from the Edin. Med. and Phys. Jour.*

This is an instructive paper. The author adverts to the discrepancy of medical testimony and opinion, in relation to the sanibility of consumption. This he thinks is owing to the want of a perfect semeiography of this disease; in consequence of which, diseases, which are not genuine phthisis, but which simulate its character, and which are more under the controul of remediate treatment, are, when cured, reported as examples of the genuine tuberculous disease. "The external signs," the author observes, "by which physicians are in the habit of recognizing the presence of consumption, may depend, 1*st.* on chronic inflammation, or other morbid conditions of the bronchial membrane; 2*d.* on ulceration, with chronic inflammation of the larynx or trachea; 3*d.* on chronic inflammation of the pleura; 4*th.* on inflammation with

suppuration of the lung; *5th.* on tubercular destruction of this organ."

The first variety he thinks is the most common; when it arises from chronic catarrh,—consisting in a chronic inflammation of the mucous membrane of the trachea and broncheal tubes, and giving rise to a muco-purulent secretion, it is frequently remediable. As the symptoms of this affection in its advanced stages, are very much like those of genuine phthisis, and consequently liable to be mistaken for the genuine disease, he thinks it quite probable that the majority of the cases which are cured, belong to the catarrhal kind. Tuberculous consumption is not so frequent as would appear to be thought by some. Dr. Willan, in his Reports for April, 1797, says, "I apprehend not more than a *fourth* part of the whole number of cases put down, could be referred to proper phthisis, arising from the *slow and successive suppurations of tubercles*, in strumous constitutions." So long as the inflammation of catarrhal phthisis is confined to the membrane lining the bronchial tubes, it is sanible, but when the inflammation extends to the sub-mucous pulmonic tissue, producing a hardening of the cellular membrane, the disease assumes a fixed character, and resists the usual remedies.

Symptoms of phthisis, depending on ulcerations in the trachea and larynx, are not uncommon. The termination of these cases is *generally* unfavourable; yet cases of this variety are sometimes cured. "In a case related by Heller, in Thomann's Annals, recovery took place, under the use of sulphuret of antimony and hyoscyamus, with a restrained diet, composed chiefly of Iceland moss, and afterwards with inunction over the throat, of an ointment containing calomel and opium." Symptoms of phthisis may arise from inflammation of the pleura, extending to the lungs, producing infiltration, hardening and ulceration of its substance. Chronic inflammation of the pleura is generally attended with the secretion of a sero-purulent fluid, which often accumulates to such an extent as to compress the lungs of the affected side

into a small and compact mass. This is what Broussais calls an *atrophied lung.*

The formation of distinct abscesses of the lungs, according to Laennec, is exceedingly rare. This writer says, "that small abscesses (unconnected with tuberculous softenings) are found in the pulmonic tissue not above four or five times, and an extensive one not above once, in many hundred cases." The author thinks it probable, that cases of this kind are in some instances remediable. With regard to scrofulous, or genuine phthisis, the author thinks that cases of recovery must be exceedingly rare. This essay contains many interesting observations, and deserves to be read with attention.

Art. 5.—*Operation performed and recommended for the cure of imperforate anus; two cases detailed, illustrative of the method and success of it. By John Rhea Barton, M. D. Surgeon of the Pennsylvania Hospital and of the Philadelphia Alms-House Infirmary.*

In the cases reported by Dr. B. "there was not the slightest trace of an anus in *situ naturali;*" but the rectum terminated in the vagina.

Case 1.—The subject of this case was six weeks old. Not the least trace of an anus was seen, the fæces were discharged per vaginam. A fistulous passage existed between the vagina and the rectum. The usual operation for imperforate anus was performed. An opening was made into the rectum, through which the fæces escaped freely. Patent lint dipped in sweet oil was then introduced into the opening. Granulation and cicatrization soon commenced. It was prevented from closing, by introducing a bougie and bringing it out of the vagina by the original passage. This produced much irritation. The child was taken from town and the artificial opening closed up, rendering the operation ineffectual. Nine months afterwards Dr. B. performed the following operation on this patient. He laid open the vagina and integuments as far back as the place where the anus should be; at this place he removed a small portion of the integuments, and dissected down

until he arrived at the termination of the rectum, which he opened freely. By this operation the anterior boundery of the incision was formed by the fistulous opening into the vagina, and posteriorly it terminated at the place where the anus should be. The subsequent treatment consisted in endeavouring to promote granulations and cicatrize the original opening. The operation was successful.

Case 2.—This case was similar to the one just mentioned. Dr. Parish operated in the following way: "A probe, having a curvature suited to the purpose, was introduced into the vagina, thence through the orifice of the communication into the rectum, so as to protude the integuments in the place where the anus should have been. An incision was then made upon the point of the probe, with a small scalpel through the integuments, and the opening thus formed was freely enlarged, both anteriorly and posteriorly, until it was supposed that the intestine was opened." It was found, however, that the rectum was in fact not opened; and the operation did not succeed. Dr. P. afterwards operated according to the mode adopted by Dr. Barton, and he succeeded perfectly. These are interesting and instructive cases. The success which attended Dr. B.'s mode of operating is a sufficient proof of its importance. We well recollect the difficulties which we once encountered after performing an operation for imperforate anus in the common way. It proved impossible to evacuate the fæces, except by the daily introduction of a director, until, finally, the child died of convulsions from malformation of the heart. We shall, certainly, therefore, adopt the method recommended by Barton at every future opportunity.

Art. 6.—*Case of adhesion of the sides of the uterus. By Joseph Wheelwright, M. D. of Westmoreland County, Virginia.*

Forty-eight hours after labour commenced, it was found, on examination, that "a hard tumour, of an uniform surface, from one and a half to two inches in diameter, occupied the place of the os tincæ. Its adhesions to the whole circumference of

the vagina were distinctly traced. No vestige of the os tincæ remained." The pains were for some time suspended by opiates; and, in the meantime, it was determined to form an artificial opening for the passage of the child. A crucial incision was accordingly made through the tumour, "which was hard and nearly an inch in thickness." The hæmorrhage was trifling. The child's head was felt presenting." The labour advanced rapidly, but as laceration at the extremities of the incision had commenced, and as there was no prospect of the parts dilating, it was thought necessary to lessen the child's head." This was done and delivery was soon effected. The woman recovered in the ordinary time.

Art. 8.—*Case of Empyema successfully treated. By Dr. Craven of Harrisonburgh, Virginia.*

There is nothing novel presented in this case.

Art. 9.—*Review of a Practical Essay on Typhus Fever. By N. Smith M. D. &c. &c.*

This work is reviewed in the present number of our Journal.

The remainder of this number is occupied by reviews of the New-York Medical Repository, Vol. viii. No. 1—2.—The New-England Journal of Medicine and Surgery for January, 1824.—The New-York Medical and Physical Journal for March, 1824, and the Philadelphia Journal of Medical and Physical Sciences.

ART. IX.—*Jahres-Bericht ueber des clinische chirurgisch-augenærztliche Institut der Universitæt zu Berlin, (fur* 1821) abgestattet von Director der genanten Anstalt; Geheim. Rath. GRAEFE.

Annual Report of the Clinico-chirurgic Eye Institute of Berlin. By Professor Graefe.

DURING the year 1821, nine hundred and ninety-six individuals were treated in this institution. Of these six hundred and seventy-five were affected with surgical diseases, and three

hundred and twenty-one with diseases of the eye. Two hundred and eighty-eight operations were performed. Of the whole number of patients, only eighteen died.

We notice the following cases, as among the most remarkable mentioned in this report:

1. A young man was admitted into the institute, with a very singular disease of one of his eyes. A number of hairs grew out of the globe of the eye, near the junction of the sclerotica with the cornea. The small lipomatous (lipomatœse) surface out of which they grew, was removed with the knife, by which the patient was effectually relieved.

2. A patient, whose eyes were nearly destroyed in consequence of the irritation produced by a double row of eye-lashes, was cured by cutting off the edges of the eye-lids with a pair of curved scissors, so as to remove entirely the offending eye-lashes.

3. In a case of inguinal hernia, the protruded intestine was so much distended, that even large incisions into the ring did not enable the surgeon to return the bowel. As it was perceived that the contents of the protruded intestine were of a liquid consistence, the operator resolved to puncture the tumour with a trochar, and to evacuate its contents in this way. This was done; the bowel immediately collapsed, and, having previously passed a thread through the mesentery, it was returned without the least difficulty. On the ninth day, the thread was removed, and the patient was perfectly restored at the end of the fifth week.

4. Two operations were successfully performed, for the removal of portions of the lower jaw-bone. *Case 1st.*—A woman, aged forty-four, had suffered for a considerable time, with an osteo-carcomatous affection of the lower jaw. The whole anterior portion of the bone was very much enlarged by the disease. The soft parts over the diseased bone, were covered with cancerous fungosities, divided by deep fissures, discharging an ichorous fluid. The operation was performed in the following manner: An incision was made through the middle of the lower lip, downwards, and continued to within

a very short distance from the throat. The soft parts were then dissected off from the external surface of the diseased bone; next the boney tumour was separated from its adhesions on the internal surface, and a leather strap drawn through, under it. The maxillary was now divided with a saw on each side of the tumour, and the diseased portion, together with the teeth, removed. The hæmorrhage was stopped; the lips of the incision brought together, and held in union by means of twisted sutures. The wound was entirely healed by the end of the fifth week after the operation. The chin was, at first, flaccid and wrinkled; by degrees, however, it acquired a cartilaginous firmness. The patient's health was entirely restored. *Case 2d.*—In this case, more than one half of the lower maxillary was diseased; the disease extended, on the left side, as far as the condyloid process. The osteo-carcomatous tumour protruded downwards, towards the throat; it was also much enlarged posteriorly, and firmly pressed against the important vessels and nerves of the neck. The internal cancerous fungoid tumours had pushed the tongue into the right cheek, and had filled up the cavity of the mouth so much, that the patient could only, with the greatest difficulty, take small quantities of liquids; articulation was almost entirely destroyed, and respiration was performed with the utmost labour. In this situation, the patient urgently requested that the diseased parts should be removed by an operation. The circumstance of the intimate connection of the enormous tumour with the important surrounding structures; of its insinuation between the large blood-vessels and nerves of the neck, and of the hæmorrhages which immediately ensued on disturbing the fungoid excrescences, rendered the danger of this undertaking, of which surgery, as yet, afforded no example, at once evident and appalling. As it was, however, certain that the patient could live but a short time, unless relieved by an operation, the surgeon thought it his duty to comply with the earnest desires of this heroic girl. The operation was performed in the following manner: The first step

was to take up the carotid artery, to obviate dangerous hæmorrhage. When this was effected, an incision was made through the cheek from the corner of the mouth to the posterior edge of the inferior maxillary; a second incision was then made, circumscribing the diseased portion of the skin, from the beginning of the first incision to its termination. A third division was now made from the beginning of the two first incisions, in the direction of the articular process of the maxillary, and carried over the joint to within a short distance of the ear. Having made these divisions, the external surface of the diseased bone was separated from the soft parts; it was next, also, detached at the chin, from its internal connections, and immediately divided, at that part, with a saw. The bone was then dissected from its attachments posteriorly, separated from its articular cavity, and removed. After uniting the wound, at the angle of the mouth, by proper sutures, the parts were covered with simple dressings. The patient was quite well during the first seven days after the operation. On the night of the eighth day she was suddenly seized with apoplexy. The effects of this gradually changed to a state of fatuity, attended with hemiplegia, and an inability to speak. The paralysis of the right foot gradually subsided. The healing of the wound, notwithstanding, went on regularly, and cicatrized perfectly. The paralysis of the right arm and tongue also disappeared by degrees. The patient began to recover strength, and enjoyed, when this report was drawn up, a good state of health. E.

ORIGINAL DEPARTMENT.

(We have been favoured with the following communication from our friend, Professor Hoecker, of Berlin, whose character as an erudite writer is fully established in Europe. The work on the History of Medicine, which he is now publishing in annual volumes, ranks among the classical productions of the medical literature of Germany. The doctrines which he here advocates, are taught, in his lectures, at the celebrated university to which he is attached. They will be interesting to our readers, inasmuch as they may be considered the standard of medical orthodoxy in one of the first schools on the continent of Europe. The manuscript is sent to the press exactly as it came to our hands from the learned author himself, without any alteration either in style or orthography. It will serve to show not only the thorough knowledge which the German professors have of our language, but also the probability that all our productions may pass under their immediate observation.)

ART. X.—*Observations on the Operation of Medicinal Substances.* BY F. FR. CH. HOECKER, M. D., Professor of Medicine in the University of Berlin.

(Communicated for the Medical Review.)

THE action of remedies has, in all ages, been generally explained by resorting to arbitrary suppositions; and hence each particular school has modelled this doctrine conformably to its own peculiar tenets. It was reserved for the sagacity of modern times to adopt the only certain method which can lead us to a knowledge of the laws of organic life, as connected with this subject; an inconsiderate adherence to all the parts of a pre-embraced system never conducting to useful results. And, we may ask, what benefits were ever likely to be derived from the views of the humoral, chemical, or even mechanical pathologist? views depending on false notions of life, and which, like shadows, disappeared as soon as the light to which they owed their illusive reality, was made to shine on an opposite side of the subject. Even the fundamental ideas to be found in more modern systems, based on dynamic principles, have proved so little satisfactory, that many totally despair of our ever being able to arrive at certainty on this head. And thus, errors, tending in no small degree to impede our further advancement in the knowledge

of therapeutics, both in its specific and general doctrines, have been suffered to remain unrefuted; nay, not seldom, have been generally received as incontestible verities, to the great prejudice of future inquiry. In our present lamentable state of ignorance on this subject, isolated explanations and rectifications is all that can be expected: but where fragments alone are possible, there all possible fragments make up the whole.

The theory of absorption, as established by modern experiments,* offers to us a certain fixed point, whence we may commence our researches. It was generally believed, that the absorption and action of substances introduced into the body, was exclusively confined to the lymphatic system. Even previous to the times of Mascagni and Cruikshank, the old doctrine of venous absorption had been pretty generally rejected, and afterwards appeared to be universally given up. Little did physicians imagine, what a sensible restriction they thus imposed on medicine, but they soon found themselves involved in difficulties respecting the explanation of several important occurrences in the animal frame, such, for example, as the rapidity with which many substances pass off by the kidneys, the actual translation of matter in metastases, and even the operation of medicines themselves, having quitted a plain and beaten path, for one more remote and obscure, the thorny narrowness of which they never once seem to have remarked. At length it was found necessary to restore to the veins their ancient office, and hence alone are we capable of taking a sufficiently comprehensive view of the above-mentioned processes, corresponding to the various functions discharged by the veins. We may assume, then, as an established fact, that there are two ways by which

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* On this subject compare: F. Tiedemann and L. Gmelin versuche über die wege auf welchen substanzen aus dem Magen und Darmcanal ins Blut gelangen, uben die verrichlung der Milz und die geheimen Harnwege. Heidelberg, 1820, 8vo. and an essay by Seiler and Ficinus, inserted in the Zeifschrift fur Natur und Heilkurde, where will also be found a full enumeration of those who have treated upon this subject.

substances enter the system; the one by lymphatic, the other by venous absorption. But these differ materially from each other; a circumstance determinable a priori, from a knowledge of the fact, that from a difference of structure results a difference of function. The most important one for our present purpose, is the following, viz., that the veins are destined to take up the more heterogeneous substances, the lymphatics, such as are more immediately proper for nutrition. Medicinal substances, in general, belong to the former class; consequently, their effects must obviously depend on the laws of venous absorption, whence alone they are to be explained.

1.—Conformably to the above statement, we very naturally first inquire into their action on the blood,—that fluid organ, itself possessed of a high degree of vitality, and transporting to every part fresh organic matter, in which the vitality of the organism takes root, not as the plant in the dead earth, but as the fruit in its maternal womb.

The effects produced on this fluid by medicines, are no doubt as various as the substances themselves; but the want of a sufficient number of accurate and diversified experiments, presents here serious obstacles to the success of our investigations. A mere change in the intensity of the vital actions of the blood, will not carry us far in our explanations, and appears only in a few instances to throw light on the operation of medicines. It is, however, this variously modified activity of the blood, inexplicable on the mere supposition of increased or diminished action, which it is here necessary to understand, but of which we are unfortunately the most ignorant. If what is termed the putrid, scorbutic, and dissolved state of the blood, be admitted as a proof of diminished vitality; and, on the other hand, an increased tendency to coagulation, a greater adaptation for nutrition, be regarded as a mark of a higher degree of vital energy, then the modus-operandi of such substances as produce these states, will, for the most part, at once be evident. Resolvents, especially those of an alcaline nature, act by being directly received into the blood, diminishing its vitality, and impart-

ing a tendency to dissolution. Tonic antiseptics, on the other hand, have a diametrically opposite action. Here, however, it is of consequence not to lose sight of that vital property of the blood, by which it withstands the action of foreign bodies, and by which no doubt the virtue of remedies is often completely overpowered, so that it then merely serves as a vehicle of transportation, thus conducting them, itself undecomposed, to other parts of the system, upon the specific character of whose vitality they are calculated to act. If the substance be of a nature altogether heterogeneous to that of the animal frame, as is the case with the metals, we then find, leaving out of consideration its action on other parts, that the vital energy of the blood is sometimes raised to the highest pitch; agreeable to the general law, the stronger the action exerted by any substance, the greater the effort of resistance. But should this increase of action eventually prove insufficient to prevent the action of the foreign body, then the blood passes into an opposite state, just as a high degree of excitement of the frame, in general, is followed by one of corresponding depression. Mercury given to a certain extent, brings on a true inflammatory state of the blood; for when drawn in the height of salivation, it has often been found to present the buffy coat; if its use, however, be pushed still further, a dissolved and scorbutic condition of that fluid is the consequence, as daily experience witnesses.

Of much importance is it, in the next place, to consider the actions of the liver, which evidently exerts so powerful an influence on the blood, that we cannot but look upon it as the most important organ of assimilation, next to the lungs. It affords another method of effecting the decarbonization of the blood, to express the real nature of its function according to present chemical views; and though the process is different, it is not less extensive. Substances, therefore, which have passed through this organ, are consigned, materially changed, to the inferior vena cava, whence their subsequent effects can only have a more or less distant correspondence with their original character. The assimilation of medicinal

substances, indeed, commences with their absorption, and their action on remote parts depends on the degree to which that process has been carried, according to which they retain more or less of their specific nature; and consequently, also, on the nature of the elements into which they may have been decomposed, as being either of a mild and completely organic character, or still capable of acting as heterogeneous substances. Sublimate is no longer sublimate after having passed the stomach, yet the effects of its elements,—effects of the sublimate after suffering organic influence—are easily perceived on the most distant parts of the system. Of course, the liver is not a mere conduit through which these substances pass, but is itself modified by them, and these modified actions naturally evince themselves more directly in the biliary secretion. This puts into our hands a means of re-acting on the alimentary canal, although it mostly happens that the effects we are speaking of, are somewhat obscure, and thence less open to the eye of observation. We must remember that the liver is a venous organ, in consequence of which its actions are tardy. The prevailing character of its diseases afford a sufficient proof of this. Its irritability, moreover, not being great, and the effects of substances being more evident in organs possessed of a higher degree of this property, and which carry on their functions with greater activity, it is easy to conceive why so few of them, comparatively speaking, seem to possess any particular action upon it. Further hepatic disorders from the nature of the organ being with difficulty removed, the want of confidence which physicians have generally placed in aid sought from this quarter, seemed confirmed by experience.—It must, however, be confessed, that the excessive doubts and scruples of many on this point, have little contributed to the advancement of medical science. Conformably, then, to the preceding observations, it must be granted, that medicines, used internally, possess certain direct actions on the liver.

2.—The most important of all organs, however, with which medicinal substances are brought into actual contact,

is the heart, the influence of which is also, at the same time, no doubt the most extensive. The blood, its natural, almost exclusive, and never-failing stimulus, surpasses all sympathetic ones, although in point of intensity the latter may hold the next rank. The extremely powerful action of foreign substances on this fluid, is most strikingly exemplified by injection, a rude and dangerous manner of operating on the animal organism, which can never afford us sufficiently general results, but must ever continue restricted to a few individual cases.

Substances thus introduced into the blood, do not undergo the usual and gradually assimilating processes, but without suffering any change of property, are at once brought into contact with the heart, just as we received them out of the hand of nature, or as they were presented us by the crucible or retort. Less perturbating, indeed, is the operation of strong remedies, after being more or less decomposed and assimilated; but still sufficiently considerable, from the great irritability of the heart, to furnish us with a most powerful means of producing salutary changes in the animal economy, when conducted with judgment, and a knowledge of the reciprocity of functional actions.

It is well established, that any change in the heart's action is propagated to the extremity of the arterial system. In fact, the specific character of its vitality depends nearly entirely on the state of the heart. The motion of the blood in the arteries corresponds to the manner in which the motions of the heart are regulated, whether strong or weak, frequent or rare, quick or slow, and can indeed be but little affected by the arteries themselves. As the heart would seem possessed of the highest possible degree of irritability, so the arteries have only the much inferior property of contractility; the former acts immediately on the application of stimuli, the latter slowly and gradually. It is of consequence here to distinguish between the mere mechanical or hydraulic, and the dynamic influence of the heart upon the arteries. The former manifests itself in the manner in which the blood is

propelled through the arteries, and the effect of the stronger or weaker columns of blood upon the latter; whence results the influence of the varied force of circulation on the functions of individual organs. The latter, or dynamic, on the other hand, displays itself in the influence of the heart's irritability on the contractility of the arteries. Both disease and the effects of remedies, which indeed generally throw a mutual light on each other's actions, present us with numerous instances of these two species of influence.

Spirituous substances, which remain in the blood for a considerable length of time, without suffering decomposition, act as violent stimuli of the heart, increasing the frequency, quickness and strength of its contractions; whence the circulation in general becomes accelerated, and the vital actions of the whole frame raised to a higher pitch. This is clearly evinced by augmentation of the animal heat, by the activity with which the various functions are performed, as well as by a feeling of greater animation, experienced by the individual himself. Regarded in this light, the action of such substances on the nervous system, is merely secondary; although we cannot deny, that in part, and only in part, as we shall see presently, it is primary; whereas, formerly their action has in general been exclusively limited to the nervous system.

In the present, as well as other instances, the two sides of the heart are manifestly not equally affected; since previous to the arrival of substances in the left side, they must necessarily be subjected along with the blood to the assimilating processes carried on in the lungs, where they no doubt undergo important changes, and become diminished in activity. Hence it follows, that the left side will be much less exposed to their action than the right. This, however, can be of little moment where action is merely exalted or depressed; for the contractions of the heart in all their extent, are regular and synchronic, so that a stimulus acting on any particular part, exerts its influence over the whole, just as well as if it had been brought into actual contact with its entire surface.—

Directly opposed to the action of the volatile stimulants, is that of the digitalis purpurea. This latter produces an absolute diminution of the heart's actions; whence it moderates the force of circulation and the functions depending thereon. In some respects, the mineral acids bear a resemblance to this substance; both diminish the frequency of the pulse, but in a very different manner. The acids, not by lowering the irritability of the heart, but by increasing the intensity of its contractions, so that their repetition within a given period becomes less than when excited by a more inconsiderable or moderate force. In short, in proportion as their vigour is augmented, they become more regular and exact. But this addition of energy to the heart is not confined to that organ alone; its influence is extended over the contractility of the arteries throughout the system; the pulse becomes harder, the vessels contract themselves more than usual, and in consequence thereof, hæmorrhage sometimes occurs from their extremities. Between this influence of the heart on the arterial system, easily observed also in other circumstances, besides those of increased power of contractility, and the phenomena of sympathies, generally attributed to the nerves, we may have a very evident analogy. To ascribe the above effects to the presence of the acid in the arterial blood, is a supposition scarcely admissible, even though we should grant what may be termed a higher state of oxidation of that fluid, and therefore one more calculated to act on the contractile power of the vessels. In our opinion, it is much more natural to explain it on the principle, that similar vital phenomena in an organism, whether produced in conjoining or distant parts, remain still in connexion, and reciprocally act on each other. The existence of this consent between the irritability of the heart and the contractility of the vessels, cannot, we presume, be called in question. They are properties differing only in degree, not in nature; and the very marked distinction made between them by Haller, and still more, his terming contractility a vis mortua, have but little contributed to aid

the labours of his followers. From the latter errors, however, physiologists appear to have timely freed themselves.

Hitherto the effects which we have mentioned as resulting from substances brought into contact with the heart, are of a general kind; that is to say, extending regularly over the whole frame, no part being more particularly affected than another, except, perhaps, where, from a higher degree of vascularity, a larger surface of action is presented. There are, however, many remedies which seem to operate in a more especial manner on particular parts; and which, notwithstanding they are carried along with the circulating blood to the heart, have little or no effect upon it; but suffering more or less change as they pass along, arrive at certain organs, upon whose irritability they appear to be calculated to act. Such effects are termed specific; although it is usual to denominate remedies specific which are useful against certain diseases, without any reference to the seat of such diseases.

Of all specifics, mercury incontestibly claims the first rank, as a stimulus of the salivary glands. There is none, however, respecting the operation of which physicians have been more greatly mistaken, owing to the erroneous notions prevailing on this subject. Agreeable to the doctrine previously laid down in the commencement of this paper, and which is established by most unexceptionable experiments, and therefore needs no further elucidation in this place, namely, that heterogeneous substances are taken up by the veins, there can be no doubt that mercury acts through the medium of the blood. The absorbents, so termed by way of distinction, do not take it up, or at least in so small a quantity, as to be totally unworthy of consideration. We have already hinted, in passing, at its action on the blood; that it further acts on the irritability of the heart, is clearly evinced by the well known fever to which it gives rise. Its effects, moreover, on the extremities of the arteries, or what is termed the capillary system, and thence on the organizing process altogether, are very remarkable. It both accelerates and alters secretion, either in the same or a similar manner to that by which nature endea-

vours to rid herself of foreign matters: and we imagine, that it is thus alone that she removes general affections of the system, from whatever kind of virus they may originate, and, above all, by increased perspiration. At the same time, absorption goes on with increased activity at the venous extremities, to which, perhaps, together with altered secretion, the resolvent effects of mercury are to be attributed. Its antisyphilitic virtues would appear to be solely ascribable to its rendering the system capable of throwing off the absorbed virus, by the complete change effected in its reproductive actions, of which increased secretion is a consequence. For we know, that by keeping up a proper degree of perspiration for a sufficient length of time, the same end may be accomplished, even without mercury. This, however, is no disparagement of the virtues of that remedy, which manifest themselves, even under unfavourable circumstances.

If, then, this statement be consonant with truth, it is at once conspicuous, how ill-founded and little worth the generally received opinion is, that the operation of mercury is confined to the lymphatic system. In proof of this, it is usual to adduce salivation; although the salivary glands have but little connection with the lymphatic system, and as far as secretion is concerned, none at all.

There are still other specific effects of remedies, using the term specific as previously defined, and particularly with respect to the excretory organs. The kidneys are generally employed by nature as an outlet of heterogeneous substances, and hence the reason why they are more frequently called into action than other organs. Is this action of such a nature as to accelerate secretion, then they act as diuretics. Other examples of modified action, not merely attributable to change of intensity, are sufficiently well known. All the phenomena observed with respect to these organs, admit of an easy and simple explanation by venous absorption, and it is at the same time very visible, how little physiology has been benefitted by the various fanciful and unhappy conjectures formed on this subject, however much acumen may occasionally have been displayed.

3.—We come next to the consideration of the effects produced on the alimentary canal. The result of a series of experiments, instituted on this point, shows, that totally heterogeneous substances are but sparingly absorbed by the veins of these parts, and that by far the greatest part immediately passes off by the first passages as foreign matters. It is otherwise, however, with substances capable of being fully assimilated; such are completely absorbed. Besides straightening the passages, as it were, by which substances enter into the system, nature has also further wisely ordered, that by acting as stimulants, they promote intestinal secretion, accelerate the vermicular motion, and thus hasten their own rejection out of the body. In this manner, substances of a totally opposite nature act directly on the irritable parts of the intestinal canal, without previous absorption. The effects produced, however, are not always similar, sometimes very essentially different. Some appear to act more particularly on the reproductive process; others rather on the irritability of the canal, or, in other terms, by accelerating secretion, and the peristalic motion. And we cannot but esteem it as one of the most gross errors, originating in too great a desire for simplification, to deny the existence of such differences among the various purgative remedies. They are usually treated of more fully than other substances, by authors on general therapeutics; it would seem, however, that the idea of derivation is all that is here required to be more particularly kept in view; or, to express ourselves otherwise, the laws of consent and antagonism, especially with respect to the operation of the antiphlogistic salts. To say that a remedy has a cooling or antiphlogistic operation, gives us no clear idea of the subject; it merely informs us of a fact, to obtain which we must employ a certain means. All anti-inflammatory salts excite the actions of the intestinal canal, even when purging does not follow. To suppose that they acted as debilitants of the intestinal canal, was certainly highly erroneous, for it is precisely here that they act as stimulants, and debility is a consequence either of their abuse or protracted administration,

just as any other over-excitement is followed by a similar result. To this very excitement are due their good efforts; and which, in inflammation of distant parts, operates solely by derivation from the extensive sympathies of the alimentary canal with the system in general. If it is true, that the great art of physic consists in discovering and subsequently following the laws of consent and antagonism, or, in other expressions, the organic relations of parts, then the above doctrine furnishes a most striking proof of this truth. It may be objectingly asked, why do other evacuants not only not prove equally beneficial, but even prejudicial? The answer is contained in the explanation already given. Many bring on other effects totally inconsistent with inflammation; they heat, constringe, or the local excitement of the intestinal canal is communicated to other regions, and thus they neutralize their own salutary influence. Further, the effect of that portion of them, which is received into the blood on this fluid itself, as well as upon the heart, and thence indirectly on the vessels, must also here necessarily be taken into consideration. All neutral salts bring on a tendency to a dissolved state in the blood, prevent it therefore from taking on a highly organizing character, in which state it acts much more powerfully on the heart, and thus abstracts in part that morbid stimulus which gives rise to so great perturbation of its functions. Mercury appears to counteract inflammation in a similar manner.

4.—The action of substances on the nerves, so variously treated of by authors who have adopted dynamic principles, is capable, we apprehend, of a plain and simple explanation. After being received into the blood, they are brought along with that fluid into contact with the nerves of the whole system, so that, when the latter are susceptible of their action, the effects will be general, or of a more partial kind, from the greater distribution of nerves to some particular part. Besides this, they possess a direct and immediate action on the nerves on being taken into the stomach, previous to any absorption whatever. The impression thus received they propagate in the same manner as they do other local irritations.

To enumerate all these laws of propagation, would here be of no use; we shall, therefore, content ourselves with mentioning one which is of the highest moment, viz. that nervous irritation, of whatever kind, or wherever excited, continues to be propagated, till, by a multiplication of fibrous structure of the nerves, an obstacle to its further progress is presented, after which it necessarily becomes diversified or intercepted. Whence, it naturally follows, that the central mass is most liable to be affected, and next to this organ the cuticular surface, and in short all parts, between which and the stomach there exists a nervous consent, and which from their structure present obstacles to the propagation of the impression, especially the ganglia, and thus, by retarding it, expose themselves in a greater degree to its action. The parts contained in the chest and abdomen, are particularly of this character. All these influences are purely of a dynamic nature, there being no conveyance of any subtle matters, an idea from which, till very lately, physicians have not been able to free themselves.

Such appear to me, in the present state of our knowledge, the chief points of view, under which we are to consider the operation of remedies in general. It is scarcely necessary to mention, we presume, that the effects previously described, never exist singly, but, to a greater or less degree, conjointly. It were easy to adduce further proofs and examples, in support of our assertions, had it been consistent with the object of a mere precursory essay. Besides, when general principles are once properly laid down and well established, their application to particular cases presents little difficulty.

ART. XI.—*A case of Osteo-sarcoma of the Lower Jaw, in which the body of that bone was amputated.* BY GEO. M'CLELLAN, M. D.

SOMETIME in the spring of the year 1821, Mary Rice, a daughter of Mr. Martin Rice of this city, fell upon the pavement with considerable violence. She was taken up in a state of insensibility; her chin was severely contused, and the front

incisors of her lower jaw were found to be loosened. For a long time after this accident, she complained of severe pains, shooting along the throat from her chin to both ears. The teeth, which were loosened, soon became discoloured, and less firmly attached to the gums.

In about three months afterwards, her parents first observed a small, hard, fleshy-looking tumour underneath the apex of the tongue. The front part of the jaw, about the same time, also began to enlarge, and gradually extend itself downwards and forwards. At this period the pains became very acute, and they subsequently continued with the development of the disease.

Although the tumefaction of the bone continued regularly to increase, it did not acquire a very alarming aspect, or produce much mechanical inconvenience, until sometime in the early part of the year 1823. Notwithstanding the little sufferer experienced much pain and loss of sleep, she preserved a good appetite, with the power of masticating any kind of food. After the period last alluded to, however, the parts became so enlarged as to produce a frightful appearance, and prevent the introduction of solid aliments into the mouth. The bone not only swelled downwards into the throat, but the tumour, which had originally been discovered underneath the tongue, extended itself upwards into the mouth, and by rising above the level of the teeth, wholly prevented mastication. From this deplorable condition, the disease continued to increase, with alarming rapidity, until the tenth of July, 1823, when I was called in to examine the patient.

She was then exactly six years old. Her strength was greatly exhausted, her countenance indescribably haggard and distressed, and her body emaciated to the extreme. The whole substance of the bone, in front of its angles, appeared to be enormously enlarged, projecting downwards before the neck, and backwards into the throat. The under lip was greatly extended, and attenuated by the pressure of the enlarged bone, which directed itself somewhat towards the left side of the throat. The skin appeared very vascular, with

tortuous and enlarged veins; and numerous small arteries could be felt pulsating under its surface. Her mouth was pushed open, as much as possible, by a huge tumour, which extended upwards from the inner surface of the inferior maxilla, and protruded outwards, so as to present precisely the appearance of an enlarged tongue. (See it as represented in fig. 1 & 3.) This tumour rose above the molar teeth, so as to be applied firmly against the roof of the mouth; and it entirely overlapped the incisors as it extended anteriorly, enveloping them in its substance. There was a small longitudinal fissure in its superior surface, through which the two blackened and detached incisors had been picked out with a probe. It was impossible to discover the posterior limits of this tumour, although, by pressing it downwards with a spatula, the apex of the tongue could be seen resting upon it behind. The tongue must have been pushed backwards by it, almost into the cavity of the pharynx, as was evident from the difficulty experienced in deglutition, as well as in respiration. Nothing but fluids could be introduced into the throat, and they were swallowed with the most imminent danger of suffocation. The family had been watching for several nights, with the hourly expectation of seeing her die, in some one of the strangling fits with which she was frequently afflicted.

In this state of things, it was, of course, decided, that nothing but an operation could save her life. As the glands of the throat did not appear to be affected, and as I was confident that I could execute the mechanical part of the operation without any immediate danger, I made no hesitation in undertaking the task. Accordingly, on the thirteenth of the same month, three days after my first visit to the patient, I performed the operation in the following manner:

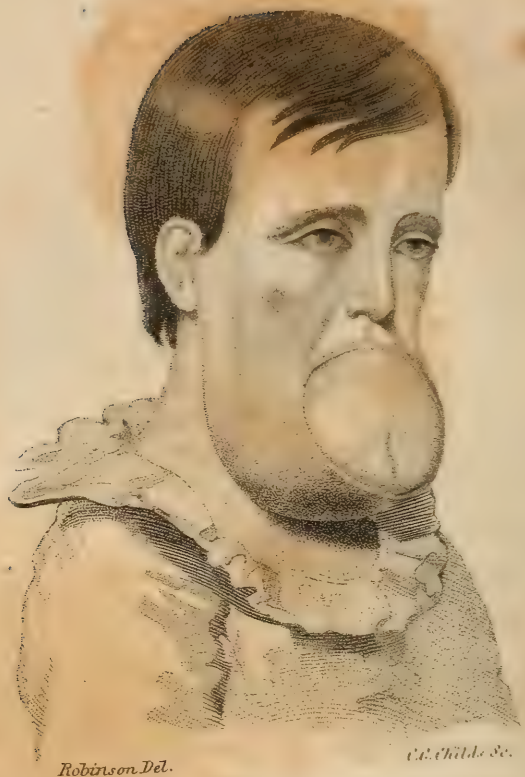
An incision was made through the integuments, from the left commissure of the lips, obliquely downwards and backwards, and carried over the anterior edge of the sternomastoid muscle, so as to command the carotid artery in case it should become necessary to secure it on that side. The anterior edge of this incision was next raised, and the low-

er part of the tumour exposed, until I ascertained that the carotid need not be interfered with. The integuments were then rapidly dissected forwards, until the whole surface of the tumour was exposed round to the opposite side. Though numerous arterial twigs were divided, I paid no attention to them; but proceeded at once to secure the facial artery on each side, just as it emerged from the sub-maxillary gland, beneath the jaw. As these supplied nearly all the divided twigs, the hæmorrhage immediately ceased. I next dissected up the insertions of the masseter muscles, a short distance behind the tumour, and exposed the surface of the sound bone, which was immediately afterwards divided on each side, by a metacarpal saw. The whole tumour was then turned outwards from the mouth, and carefully dissected from the under surface of the tongue, the sub-maxillary glands, and the muscles, on each side. A part of the sub-lingual glands, and a considerable portion of the left sub-maxillary, appearing to be tumefied and somewhat discoloured, I removed them altogether by the scalpel. Only three more arterial twigs, underneath the tongue, required the application of a ligature. Not more than six or eight ounces of blood were lost during the whole operation, which altogether occupied four minutes and a half. The little patient did not faint, although she was at first nearly suffocated by the blood which passed into her throat.

After the parts had been carefully inspected, and all present had agreed that no morbid structure was left behind, the huge flap, or rather pouch of skin, was re-applied to its former situation; and the edges of the external incision were retained in apposition by three interrupted sutures, and some adhesive plasters. The large cavity left beneath the tongue was partly filled with patent lint, bent into the shape of the lost circle of bone, upon which the pendulous integuments were afterwards lightly braced by means of a bandage.

After the dressing was completed, the tongue was discovered to be in its natural situation; and, to the great astonishment of all the family, the little sufferer began to articulate with considerable distinctness. She also called for water, and

Fig. 1.



Robinson Del. *C.A. Childs Sc.*

Fig. 2.

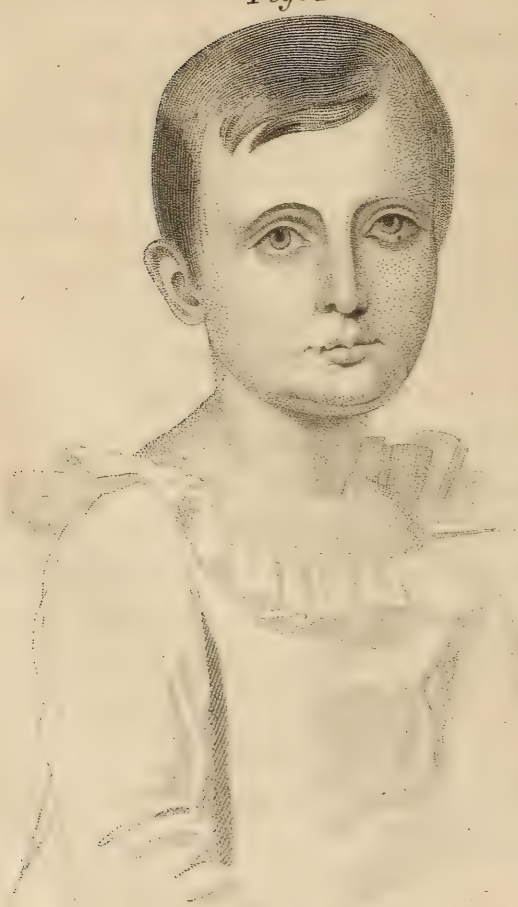
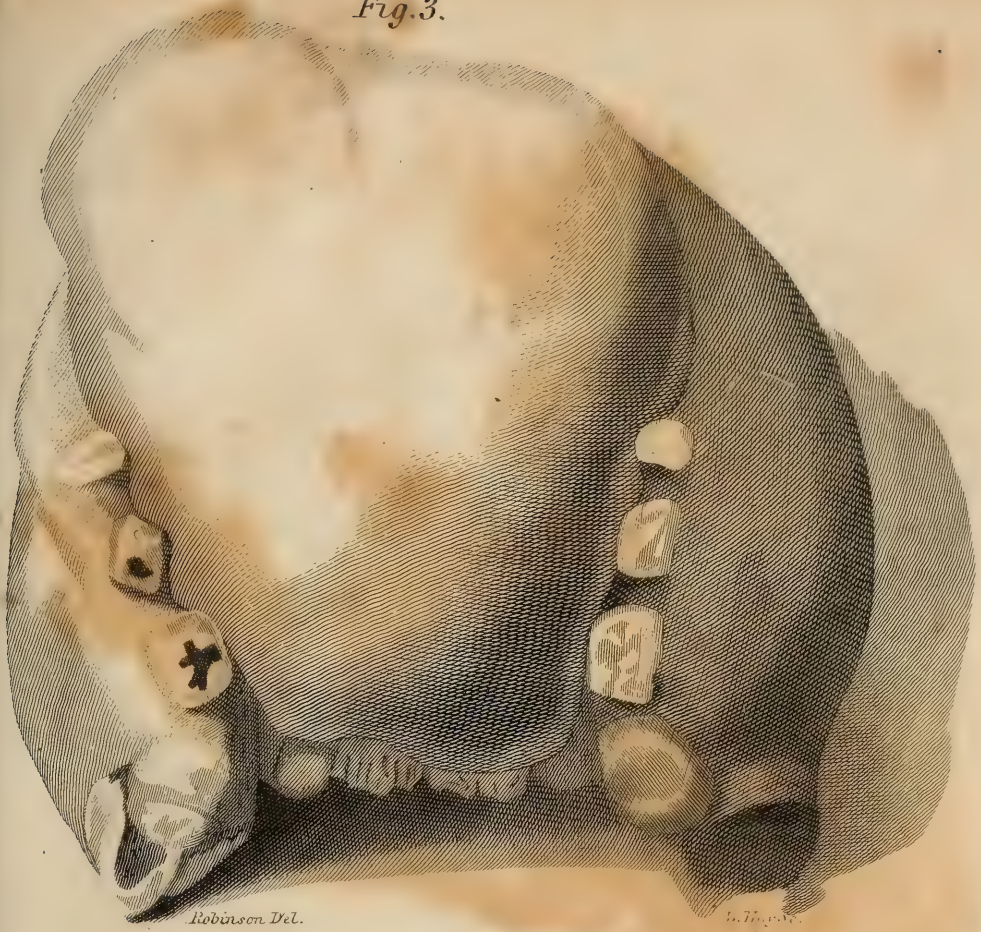


Fig.3.



drank a moderate quantity from a common cup. Indeed, both the functions alluded to, which could hardly be considered as existing before the operation, began almost immediately afterwards to be executed with comparative ease.

But very little pain was experienced, and she slept soundly through the succeeding night. Her bowels were afterwards kept open by the daily use of injections; and she was nourished with broth and mucilaginous articles. When the dressings were removed on the fourth day, the external wound had become agglutinated by adhesive inflammation; and the cavity previously occupied by the tumour, was suppurating profusely. The mass of lint was, therefore, removed, and replaced by a fresh stuffing of the same material. Granulations soon afterwards sprouted up from the surfaces of the cavity, and displaced the internal dressing, which, on the seventh day, was altogether dispensed with. On the eighth day after the operation, she was taken across the Delaware in a boat, for the benefit of fresh air. In three weeks the parts were entirely cicatrized internally, as well as externally. During this period, a luxuriant crop of granulations shot out from the cut extremities of bone on each side, which finally became ossified to the distance of about one inch in front of the angles, and formed a solid support for the soft parts below the cheeks. The new flesh which had formed beneath the apex of the tongue, also became indurated into a ligamentous mass, which gave firmness to the integuments, and bolstered out the chin to its natural prominence. The induration of this ligamentous matter, like that which occurs between the fragments of a broken patella, eventually became so great as to answer the purpose of a substitute for the bone, in the place of which it was deposited. The muscles contracted adhesions to it; and she could easily masticate the common articles of food.

In less than five weeks after the operation, the child became robust and fleshy. She afterwards grew very rapidly in height, and went to school every day in good health and spirits. Her articulation became accurate; and her counte-

nance, to the surprise of all who visited her, presented its natural appearance. The only vestige of the operation which could be discovered externally, was the linear cicatrix of the first incision extending from the left angle of the lips, obliquely downwards across the throat. There was, however, a transverse wrinkle underneath the prominence of the chin, which resulted from the contraction of the superabundant integuments. These are plainly exhibited in fig. 2., which was taken in the fourth month after the operation.

But, notwithstanding all these favourable circumstances, the final termination of her case was not happy. In four months after the operation, she contracted a severe cold, which was followed by fever. In two days after, the right sub-maxillary, and several of the neighbouring lymphatic glands, swelled enormously, and became slightly painful. Under suitable depletion, emollient applications, &c., this tumefaction speedily subsided, and the febrile symptoms disappeared. The sub-maxillary gland, however, remained in an indurated state, and the integuments of the chin began to assume a dry and corrugated appearance. Soon after, the internal surface of the cicatrix, beneath the tongue, contracted a yellowish-white colour; and the whole substance of the chin increased in thickness and density. These morbid appearances advanced with great rapidity. By the middle of January the tumefaction of the parts had acquired an enormous size, and the skin began to ulcerate externally. Deep fissures afterwards formed in the skin, which extended in various directions from the ulcerated surface, and revealed a dense white mass, of a cheesy or cartilaginous appearance, precisely like the substance of the tumour which had been removed by the operation. The latter stage of this horrid disease was still further marked by the formation of large sloughs, which penetrated through the whole thickness of the cheek, so as to expose the cavity of the mouth. Intolerable fœtor also attended, and emaciation, with extreme debility, rapidly conducted the patient to the close of her unhappy life. She expired on the twenty-second of March last, without having

suffered either severe pain, or loss of sleep, from the time of the operation, until a few days before her death.

On dissection, the following circumstances were disclosed: The heart was both enlarged in substance and exceedingly distended with blood. There were two small white cartilaginous incrustations on the surface of the ventricles. The pericardium was fully distended with a reddish serum. The lungs presented a singular appearance; about one-third of their exterior being perfectly white, while the remainder was of the usual colour. There were several small tubercles about the ramifications of the left bronchia, two of which were distinctly scrofulous. I could find no other appearance of disease in the viscera, except in the liver, which was studded in every part with white spots of the size of grains of wheat.

The third figure, engraved from an accurate drawing of the preparation, illustrates the size and form of the original tumour, together with all that portion of the bone which was amputated with it. The *cuspidati* and the *first molares*, are seen almost in their natural positions. The situation of the *third molar tooth* on each side, is shown distending the gum just before the transverse cut of the saw. The apparent enlargement of the bone is also shown on each side; as well as the form of the tumour which projected upwards into the mouth. Underneath its posterior extremity are seen the origins of the *genio-hyoidei, &c.* The two first figures are exact miniatures of the patient, taken before and after the operation. The high character of the artists by whom they were executed, precludes the necessity of any testimony respecting their correctness. It should be observed, however, that a remarkable precocity of growth, as well as of mind, gave to the child an appearance of much greater age than she had actually attained.

ANALECTA.

Radical Cure of Hernia by Bandages.—Professor Langenbeck cured Hernia effectually in six weeks, by compression: "I lay," says he, "a compress of linen against the *annulus*, and press it down firmly until the surface of the skin ulcerates. In this way a firm adhesion is effected in the ring, and the hernia is prevented from re-appearing."——

Froriep's Notizen, 3 *ter. B. p.* 304.

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New mode of treating Sarcocele, without extirpating the Testicle.—The difficulties and dangers attending the operation of castration, have often rendered a safer and less troublesome mode of cure desirable. M. Maunoir proposes a new method of proceeding in cases requiring the removal of a diseased testicle, which he has practised with success in two instances, and which appears to be preferable to castration. His mode of cure is founded on the interruption of the process of nutrition in the diseased part, by tying and dividing the spermatic artery. As soon as the testicle is brought into a state of atrophy, the process of disorganization, together with its attendant symptoms cease; and its volume diminishes to such a degree that castration becomes unnecessary.

Nouvelle methode traiter la Sarcocéle sans extirpation.—Geneve, 1821.

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Pyroligneous Acid, a useful application to phagedenic ulcers.—Dr. Schulze, of Calau, has derived much benefit from the application of this acid to indolent and plagedenic ulcers of the lower extremities. The acid is to be applied to the surface of the ulcer, with a hair pencil, once a day. The ulcer must be dressed with some gently stimulating ointment.

Rusts' Mag. XIII. 1.

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Tartar Emetic Ointment, in the Cure of Intermitting Fever.—Dr. V. Pommer, in *Hufeland's Journal for January*, 1823, has published some observations on the use of tartar emetic ointment, in the cure of intermitting fevers. He directed the ointment to be rubbed upon the abdomen, three times daily, until a crop of pustules appeared. The pustules were kept inflamed for several days, and the suppuration encouraged. Under this mode of treatment, quartans generally disappeared after the sixth paroxysm, quotidians after the second, and tertians after the third.

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The Sulphat of Quinine used in large doses.—It is stated in the first Number of the *Arch. Gen. de Medicine*, that Prof. Mathæis, of Rome, has begun to administer this salt in doses of four or five grains in a spoonful of water, three or four times a-day, during the intermissions and remissions of fever. He has found it necessary to give from fifteen to thirty-five grains at least, in two or three days, to arrest the progress of periodic fevers. In this country it has been given in smaller doses, and repeated at shorter intervals. We believe that from one-fourth to one grain has usually been administered every hour or two with success, in all such cases as are attended with complete intermissions. Like the bark from which it is prepared, it cannot be given with advantage except in a state of Apyrexia.

*** As we have already transgressed the limits which we originally prescribed for ourselves, we are obliged to defer the remainder of our Analecta for the next Number of this Journal.

THE

MEDICAL REVIEW,

AND

ANALECTIC JOURNAL.

VOL. I. SEPTEMBER, 1824. No. 2.

ART. I.—*An Elementary System of Physiology.* By JOHN BOSTOCK, M. D. F. R. S., &c.—Vol. I. London, 1824. 8vo. pp. 518.

PHYSIOLOGY has been peculiarly the field of speculation and hypothesis. The animal machine is so intricate in its structure—so mysterious in its organic functions and various sympathies, and so entirely beyond our comprehension as to the spring of all its actions, that experiment and observation could advance but slowly in the investigation of its economy. As a natural consequence of the slow pace of correct knowledge in relation to the nature and functions of this wonderful frame of vital mechanism, theory and hypothesis had, for a long time, almost an unlimited sway. The mind, impatient of the tardy progress of experience and observation, is apt to pass the legitimate bounds of inquiry, and to press forwards into the dominions of fanciful speculation, where, if it cannot find truth, it may at least be gratified by the contemplation of her semblance.

In reviewing the history of Physiology, we find that during a long series of ages it consisted of but little else than a continued succession of groundless theory and conjecture. Indeed, it was not until the time of Haller that it merited the name of a science. "This great man," as our author truly

observes, "is in every point of view entitled to the appellation of the father of modern physiology." Since his time, hypothesis and gratuitous speculation have been gradually disappearing before the light of observation and experimental research. The investigations of Hunter, Bichat, Spallanzani, and more recently those of Majendie, Philip, Bell, and others, have given an entirely new aspect to physiology. Experiment and rational induction are now acknowledged as the only proper means for advancing our knowledge in this science. Indeed, the ardour and ability with which physiological studies are at present pursued, both in Europe and in this country, are, perhaps, greater than ever, and cannot fail to advance us rapidly in the science of the animal economy.

Although physiology is at present cultivated with great success,—and although an intimate acquaintance with it is unquestionably essential to the formation of an enlightened physician, yet we cannot help remarking, that in many of the medical schools of our own country it does not receive that attention as a branch of study, which its importance merits. In our own university, especially, it has not for a few years past been touched upon, except incidentally by the professor of anatomy. We do not mention this as a fault in the present faculty of this institution. The cause lies in the professorships of the Theory and Practice of Medicine and of the Institutes being united in one person,—an arrangement which necessarily precludes, from the short period allotted to a course of lectures, an adequate attention to the latter department of instruction. We know, too, that by some of our ablest physicians no books on Physiology are put into the hands of such students as resort to them for private instruction. This is not as it should be; for nothing distinguishes so much the enlightened and scientific physician from the mere mechanical routine practitioner, as a comprehensive acquaintance with the laws and functions of the animal economy. That a man may cure diseases without being an able physiologist, is as

true as that a sailor may navigate a vessel without being acquainted with the astronomical principles of navigation. Yet who can doubt that the enlightened physiologist will, cæteris paribus, be a more successful practitioner, or that he who is initiated in the principles of practical astronomy will not be a more able navigator. But it is time that we attend to the work before us. "When we reflect," says the author," upon the distinguished share which our countrymen have borne in improving the knowledge of the animal economy, it may appear not a little remarkable, that we have no original work in our language which contains a systematic and connected view of modern physiology." This deficiency is very fully and ably supplied by our author. His acknowledged abilities as a man of extensive erudition and science, qualified him peculiarly for an undertaking of this kind. Viewing the work in all its parts,—in its arrangement, its judgments, in the perspicuity of its details, and in its materials and particular structure, we regard it as decidedly the best system of physiology extant. By this, however, we do not mean to say that it is in all respects an unexceptionable work. On the contrary, we believe it to be liable to some well-grounded objections, which the author would have done well to have attended to. There is, we think, too much discussion in the work, for an elementary treatise, and too much space occupied in reviewing and refuting the exploded hypotheses of former physiologists.

The author has not adopted any of the arrangements which have been formed on the subjects of physiology. He arranges the solids, in relation to their chemical composition and uses, under five divisions:—"the osseous matter, the membranous, the muscular, the adipose, and the cerebral." He does not, however, follow this arrangement in the description of the solids. The plan which he adopts is founded rather upon the functions which the body exercises, than upon its composition. "I propose, says he, to give an account of both the solids and the fluids as I successively treat of the

functions of those parts in which they exist in the greatest quantity or the most perfect state."

Having, in the introductory essay, given a succinct view of the most prominent physiological systems from Hippocrates down to Bichat, he enters, in the first chapter, on the distribution, and the physical and vital properties of the *membranous structure.* By membranous matter he means "not only what has been usually called membrane, but the whole of the substance which possesses the same mechanical structure, and the same chemical properties." It corresponds nearly with the "white parts" of the earlier anatomists,—the cellular texture of Haller, the mucous tissu of Bordeu, and the tela mucosa of Blumenbach.

There is no term more frequently and yet more vaguely employed, than the word organization.—"In its most extensive acceptation," says the author, "it may be regarded as nearly synonymous with the word arrangement, signifying that the parts of the organized body are placed, according to some specifick structure, visible to the eye. Besides this kind of physical organization, the word is employed by physiologists in a more correct sense when it is applied to a system composed of a number of individual parts, possessing each of them appropriate powers and functions, but all conducive to the existence and preservation of the whole."

There appears to us an impropriety, however, in the former of these definitions of the term, because, as the author himself observes, it will apply equally to living and dead matter. A crystal, according to this acceptation of the word, is an organized body, although we undoubtedly attach a different meaning to it, when applied to living matter. We should define organization to be that peculiar combination and disposition of the elements of matter which gives it an aptitude for vital phenomena. Cuvier considers the existence of both solids and fluids essential to an organized body, "the latter being the media through which its functions are performed, and the former being necessary to

contain the fluids." "This view of the subject," says our author, "may be correct, but in forming a conception of the ultimate parts of an organized body, we must at length arrive at a *solid* fibre which contains no fluid, and which is however composed of regularly arranged particles." It may be observed, however, that even the ultimate fibre is probably penetrated with a fluid, which is essential to its character as an organic substance, and without which such a fibre, though still composed of "regularly arranged particles," is but an arrangement of dead particles, and wholly incapable of vital properties.

The author's account of the chemical composition of membrane, is very minute,—perhaps we may say, too minute in a work purely physiological.—Indeed, his predilection for chemical investigations is abundantly manifested throughout the whole work. We by no means regard the chemical nature of organic matter, as an object foreign to the physiologist; but it must nevertheless be conceded, that the information which we derive from this source, can contribute but very little to our knowledge of the proper object of physiology—the laws and phenomena of the animal system. Having described the structure, properties, and composition of membranous matter, he proceeds to a more particular account of the various forms under which it exists: these are the cellular texture; membranes which are divided into mucous, serous, and fibrous; tendons, ligaments, and cartilages; epidermis, corpus mucosum, and cutis; and the nails and hair. Membrane, our author observes, "is not possessed of any properties that are peculiar to the living system;" nor is he "disposed to admit that the cellular texture and the tendons, possess either sensibility or contractility." We cannot by any means acknowledge the correctness of these opinions. These parts must either be dead matter or living;—that they are not dead, or devoid of vitality, is obvious; they are therefore vital, and consequently possess that peculiar property which distinguishes living from dead matter. It is therefore mani-

festly wrong to say that membrane "possesses none of the properties peculiar to living matter," although it may not evince either sensibility or contractility. With regard to the two forms of membranous matter, the tendons and ligaments, it is equally erroneous to assert, as many physiologists do, and amongst them our author, that they are devoid of sensibility.—It is true that these structures may be cut, punctured, or irritated by chemical agents without producing any pain. It is equally true, on the other hand, that there is a mode of exciting the sensibility of these parts, which produces in them acute sensations of pain,—and consequently shows them to be endued with a certain modification of sensibility. If we irritate a tendon or ligament by the ordinary excitants which produce pain in other parts, no sensation will be produced. But if this same ligament or tendon be twisted or extended with force, great pain will be excited.—"It results from this experiment," says Bichat, "that the ligaments, insensible to agents which cut, tear, and disorganize them, have great sensibility to those which stretch them beyond their natural degree. They have then their mode of sensibility of relation, and this mode is analogous to their functions. Removed by their position from all exterior excitation which can chemically or mechanically affect them, they have no need, like the skin exposed to this sort of excitation, of a sensibility which transmits the impression of it. On the contrary, very subject to be distended, pulled, twisted in the violent motions of the limbs, it was necessary that they should give notice of this kind of irritation to the sensorium, whose excess might otherwise become fatal to the articulation. Let us remark from this example, that we should never pronounce on the sensibility of an organ without exhausting on it every means of irritation."*

Having considered the membranous matter under all its various relations, the author, in the next place, treats of *Bone.*

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* Treatise on Membranes.—Art. IV. § 5.—183.

It is only within the last forty years, that the chemical nature of bone has been well understood. Gahn was the first who discovered that the earth of bones is the phosphate of lime. For a knowledge of the nature of the animal matter of bones, we are indebted to Mr. Hatchett; he found it to possess the character of condensed albumen. The author's account of the composition, structure, and formation of bones, is very minute, and embraces all the most authentic information extant on the subject. Duhamel thought that bones are formed of concentric laminæ, which are deposited from the periosteum; and the results of his experiments, as he reported them, were generally conceived to afford decisive evidence of the truth of his hypothesis. "We are now assured, however," says the author, "that the succession of differently coloured rings which Duhamel described, could have no existence, or that if any thing resembling them took place, it could have no connexion with the periods during which the madder had been given or withheld." This hypothesis is satisfactorily controverted by Haller, who gave the proper explanation of the manner in which madder communicates its colouring principle to bones. He supposed that this phenomenon depends on the affinity "which exists between the colouring matter and the phosphate of lime,"—an opinion which has since been confirmed by the experiments of Rutherford, and Mr. Henry of Manchester.

The next subject treated of by the author, is *muscle*. He enters into a long and rather tedious account of the various opinions that have been published in relation to muscular structure. He is inclined to adopt the doctrine of Carlisle, on this subject, which alleges that the ultimate muscular fibre is a solid cylinder, the covering of which is a reticular substance regularly granulated and of very little cohesive power when dead." According to Mr. Bauer, whose microscopical observations have lately been employed on various physiological subjects by Sir E. Home, it appears that the globules of blood, when deprived of their colouring matter,

were of the same diameter with the ultimate muscular fibre, "and that the fibre is, in fact, composed of a series of the globules arranged in straight lines."

On the chemical composition of muscle, the author is minute. By digesting muscular substance with sulphuric acid, and afterwards removing the acid by the carbonate of lime, it becomes converted into "a peculiar extractive matter, from which, by means of alcohol," a substance is procured which, from its white colour, has been named *leucine;* and this substance, by subjecting it to the action of nitric acid, is converted into "what has been called nitro-leucic acid."* *Adipocere* is also obtained by digesting muscular fibre in nitric acid. This substance differs from muscular fibre by its containing more oxygen, and less azote and carbon than muscle. Muscle, under certain circumstances, undergoes a peculiar change in its chemical composition; acquiring the character of a saponaceous compound, consisting, as has been ascertained, of ammonia united with *adipocere.* "There was," says the author, "an immense burial-ground in Paris, called La Cimetiere des Innocens, which, in consequence of some improvements that were going forwards, it was determined to remove. This place had been the receptacle for a considerable part of the population of Paris for several centuries. The number of burials was supposed to be some thousands annually; the bodies were deposited in pits or trenches about thirty feet deep, each capable of holding from twelve to fifteen hundred bodies, which were covered with a few feet of earth, so that the whole area, occupying about seven thousand square yards, was converted into a mass, consisting principally of animal matter rising several feet above the natural level of the soil; upon opening the ground for the purpose of removing this prodigious collection of dead bodies, they were found to be entirely altered in their nature and appearance. What had formerly composed the soft parts of the body, was converted

* Ann. Chim. et Phys. t. xiii. 118, et seq.

into an unctuous substance, of a gray colour, and of a peculiar but not very offensive odour. According to their position in the pits, and the length of time they had been deposited, the bodies had undergone this transformation in a more or less perfect manner. The transformation was found to be most complete in those bodies that were the nearest the centre of the pits, and when they had been buried about three years; and in these cases every part, except the bones, the hair, and the nails, seemed to have lost all their specific properties, and to have acquired those of this peculiar substance."

Having explained the nature of the physical extensibility and elasticity of the muscular fibre, the author gives a well condensed and satisfactory account of the doctrines that have been advanced in relation to their vital contractility. Haller was the first who clearly comprehended the nature and extent of this property. He pointed out in what respects muscular contractility differs from nervous sensibility. The difference between the effects of elasticity and contractility were formerly not attended to. "Even Hoffman, Boerhaave, and Cullen," observes our author, "were not sufficiently aware of this distinction." The following distinctive circumstances are pointed out by the author:—"Elasticity always depends upon simple reaction, and is never the source of actual power; it merely restores, in a contrary direction, the force which had been impressed; and even when acting to the greatest advantage, the effect which it produces can never be greater than the amount of the cause, and the reaction can never take place as long as the cause continues to be applied. But in muscular contraction we observe a very different train of events. The mechanical effect is infinitely greater than the mechanical cause producing it, and indeed bears no physical proportion to it, while at the very time that the cause is applied, and is acting with all its force, the reaction commences and far surpasses the force of the agent." Our author, in common with many other physiologists, considers muscular

relaxation as the effect merely of a cessation of contraction. When relaxed, he supposes the muscular fibre to be simply in a passive state. Bichat maintains a contrary opinion; he thinks that relaxation is, in part, at least, an active effect, and that it consists in something more than in mere relaxation. This is certainly the case with the heart, and perhaps in a degree also with the other muscles. Our author's explanations on the use and mechanism of muscles, are at once clear and concise. On the various hypotheses that have been invented concerning the nature of muscular contraction, he is, we think, much too prolix.—It is certainly not in place in an elementary work of this kind, to enter into a refutation of doctrines which every one would now at once pronounce to be erroneous; or to waste much time upon inquiries concerning the nature of phenomena which are obviously beyond our scrutiny. He passes in review the doctrines of Borelli, Stewart, Willis, Bellini, Mayow, Keil, Valli, Prochaska, and of Blane, on this subject. None of these hypotheses, however, as the author observes, deserve any confidence, and he does not himself propose any other on this subject. Having given an account of the structure of the muscular fibre, its chemical nature, its properties, uses, mechanism, and the nature of its action, he passes on to consider the question: "What is the cause of contractility?" This, like the preceding topic, is one upon which a great deal of idle speculation has been advanced. The author gives a sufficient view of the principal doctrines that have been published on this subject. He arranges the hypotheses on this head, under two divisions. *First*,—"Those which ascribe muscular contractility to the presence of some extraneous agent, or power superadded to the animal body; and *Secondly*,—Those which ascribe it to some peculiar state or function of the body itself." Among the first are those which ascribe muscular contractility to oxygen—to caloric or to electricity. Among the hypotheses that come under the second of these heads, that which ascribes contractility to the chemical composition of the fibre,

has had many and very able supporters. "It is found," says the author, "that a certain proportion of chemical elements composes a body endowed with certain properties, and whenever the the elements are put together in a proper proportion, these properties are the necessary result. The same reasoning is applied to the muscular fibre; this body is composed of carbon, azote, and oxygen, which all of them exist in a certain proportion, and when they are united together they form the body which we call a muscular fibre, which possesses a certain set of physical and chemical properties, and also the physiological property of contractility. There is, however, an obvious and well known fact," observes the author, "which is decisive against this supposition, viz: a dead muscle has precisely the same chemical composition as a living one."

The attraction between the particles of muscular fibre, is considerably greater during life than immediately after death. When a muscle is ruptured during life, it is generally the tendinous part which gives way, while, on the contrary, after death, the fleshy part is always weaker than the tendon.

Having completed his account of muscular structure and its properties, the author passes on to the consideration of the *nervous system.* We regard this as the best chapter in this volume, both in relation to the ability with which it is drawn up, and the intrinsic interest of the subject itself. The first section embraces a short description of the nervous system, in which every thing is noticed that has hitherto been ascertained in reference to the structure and arrangement of this system. In section second the author treats of the "*vital powers or faculties of the nervous system, and the mode of their operation.*"

"Physiological sensibility," observes the author, "may be defined, the power which the nervous system possesses of receiving and transmitting certain impressions, and producing corresponding changes in the sensorium, but it is essential to notice that these two operations are not necessarily connected together, or that it is no necessary part of their sensibility

for these impressions to be perceived by the mind, or to become perceptions." Upon the question how an impression is transmitted by a nerve, three hypotheses are noticed. 1.—By means of a subtle fluid, called the nervous fluid; this is the oldest hypothesis. 2.—By vibrations, which is the theory of Hartley. 3.—By electricity, or a fluid analogous to galvanism. This appears to be the prominent doctrine of the day, and is indeed very strongly supported by the recent experiments of Wilson Philip and others. The most interesting and able section on this subject is the one which treats of the *use of the nervous system.* The author has entered fully into the opinions and observations extant on this subject. In relation to the different parts of the brain connected with different functions, he observes,

"Dr. Philip, whose experimental investigations have given him the means of forming a correct opinion on this subject, concludes that the division of the cerebral mass into cerebrum and cerebellum, has no relation to the voluntary and involuntary muscles, but may rather refer to some distinction to the different sensorial functions. The late experiments of M. Fleurens, seem to show that the cerebrum is the more immediate seat of perception, and the cerebellum of volition. This physiologist proposed to discover from what part of the brain irritation may be propagated to the muscles, to what part sensation extends, proceeding from the extremities towards the centre, and what is the immediate seat of volition when it is exercised in effecting muscular contraction. He endeavoured to ascertain these points by gradually removing successive portions of the brain, and noticing the effects of each removal, when he was led to conclude that the two first of these, or, as we may regard them, the centres of irritation and of sensation are in the medulla oblongata, at the part where the tubercula quadrigemina are attached to it. We are informed that the great mass of the cerebellum and cerebrum are not immediately concerned either in muscular irritation, or in sensation; they appear, however, to be necessary to perception, especially the former, which is also supposed to be the immediate seat of memory and intellect. The effect of removing the

cerebellum was to destroy the voluntary power over the muscles; the animal appeared to retain its volition, but had lost the capacity of exercising it. M. Cuvier speaks in high terms of commendation of the experiments of Fleurens, and it must be confessed, that if the experiments be confirmed, they will lead to very important views respecting the nervous functions."

Formerly much controversy existed upon the question, whether the powers of motion and of sensation are transmitted or furnished by the same nervous filaments.—It was observed that the mobility of a limb was sometimes entirely destroyed, whilst it nevertheless possessed perfect sensibility; and it was therefore very rationally contended, that these two functions could not be connected with one and the same nervous filaments. This conclusion is now fully demonstrated by the recent experiments of Bell and Majendie. These two able physiologists have ascertained, that if the posterior of the two roots by which the spinal nerves originate, be divided, all sensation will be abolished in the parts to which the nerves are distributed, and, on the contrary, that when the anterior roots are cut through, the sensibility will remain, but the power of motion is effectually destroyed. M. Magendie has, as he thinks, ascertained that this separation of the nervous functions extends to the spinal cord, and they are respectively confined to the two separate pillars of which this cerebral prolongation is composed. "From these experiments," says our author, "we gain an analogical argument for the same kind of separation in the brain, and this conjecture would appear to be sanctioned by the experiments of Fleurens and M. Rolando."

The series of experiments lately brought forward by C. Bell, in relation to the functions of the nerves, are highly interesting, and deserve very great attention.—From certain peculiarities of structure and distribution, observable in the nerves, he has arranged them under two divisions, each of which it would appear are subservient to distinct functions in

the animal economy. To the one class of nerves Mr. Bell has given the name of symmetrical or original, and to the other the superadded or irregular nerves.

"The first set, which might, perhaps, be called more appropriately, the general nerves, consist of the fifth pair of the cranial, and all the spinal nerves. They have double roots, one of which is connected with ganglia; they pass laterally to the two halves of the body, and they are distributed to all the muscular parts that are under the control of the will. They appear to be the organs of perception and volition, deriving, as we may conjecture, these two functions from the double roots. These we may regard as that part of the nervous system which serves the purpose of establishing our connexion with the external world."

"Mr. Bell's second set of nerves proceed by single roots from the base of the medulla oblongata, or the parts immediately connected with it; they proceed in a much more irregular manner than the former, and are distributed to all the organs which are concerned, either directly or indirectly, in the function of respiration. From this circumstance they have received the denomination of respiratory nerves, as well as of supperadded or irregular. Their course is designated by this last term, as they pass from one organ to another in the most intricate manner, connecting them together, passing across the general nerves, occasionally uniting with them, and forming the connecting link between the two halves of the body. The nerves are not under the control of the will, and are not capable of exciting perception; they are, therefore, furnished only with the faculty of transmitting the nervous influence, or with what Dr. Philip styles nervous power in opposition to sensorial."*

In the fourth section of this chapter, the author treats of *the connexion* between the nervous and muscular systems.

* "See Bell in Philosoph. Trans. for 1821, p. 398, et seq. Also, Philos. Trans. for 1822, p. 284, et seq; and Shaw, in Quart. Journal, v. xiii, p. 120; and Med. Chir. Trans. v. xii., p. 104; Lon. Med. Phys. Journal, v. xlviii., p. 343 and 457; v. xlix. p. 449."

After the distinct characters of contractility and sensibility were pointed out, and it was admitted that the former gave rise to motion and the latter to sensation, a question arose whether motion could exist without sensation; and this point "gave rise to one of the most animated controversies that ever took place in physiology." "The point in dispute," says our author, "may be thus stated; when a stimulant acts upon a muscular fibre, so as to produce contraction, does it act immediately upon the fibre itself, or does it not always act through the intervention of a nerve." Haller stood at the head of those who advocated the affirmative, and Whytt, his most powerful and zealous opponent, defended the contrary doctrine. The principal facts adduced in support of the Hallerian doctrine on this subject, are: 1,—Many parts are very contractile, although they seem to possess scarcely any nerves and sensibility, as, for instance, the heart. 2,—The muscular parts remain contractile long after they are separated from the body, and their communication with the brain is cut off. 3,—Fœtuses have been born without heads, but have nevertheless evinced a power of motion. 4,—The analogical support this doctrine receives from the constitution of the lower classes of animals, "such as the *actiniae*, and some of the larger of Zoophytes, which have contractile power without the least trace of a nervous system." 5,—It receives support from the fact, that some of the contractile organs are formed in the fœtus, as the heart, before the brain is perceptible. In opposition to these arguments, others, perhaps equally powerful, have been adduced by the neurologists. The most powerful of the arguments urged against the doctrine of Haller is the fact, "that exactly the same effects are produced, whether the stimulant be applied to fibres themselves, or to the nerve that is distributed to them." The experiments recently made with the galvanic influence, lead to the same conclusion.

"When a muscle is completely separated from the body, except by the intervention of a nerve, contractions are excited by

transmitting the electric influence through the nerve, and it is always found that if the trunk of a nerve form part of a circuit, it is not the muscles in the neighbourhood of the nerves that are excited, but those to which the nerve is ultimately distributed, however distant they may be. By reversing the nature of the experiment, the reverse effect is produced. If, instead of applying a stimulant, we make use of a sedative, such as opium or laurel-water, and immerse the nerve in it, the muscles to which the nerve is sent, lose their contractility as entirely as if the sedative had been applied to the muscular fibres themselves."

The circumstance, also, that muscular action is produced by mental operations, is strongly against the Hallerian doctrine. Notwithstanding these powerful facts in opposition to this doctrine, the late experiments of Dr. Philip afford, as our author believes, the most direct confirmation of its truth. The experiments of this ingenious physiologist, on this curious subject, "consisted in employing the corresponding muscles of the two extremities of an animal, the nerves of one of which were divided while those of the other remained entire. Both the sets of muscles were then thrown into strong contractions by the direct application of a stimulant, when it was found that the extremity in which the nerves were left entire, lost its contractile power as soon, or sooner, than that in which the nerves were divided." From this experiment Dr. Philip concludes, and justly, as our author thinks, that contractility is an inherent power of the muscular fibre, "and does not depend on any thing which is conveyed to them through the media of the nerves."

Of the Circulation.—Having described the principal ingredients which enter into the composition of the animal body, and the two general properties which distinguish the animal functions,—sensibility and contractility, the author goes on to consider those functions which are exercised by the organs of the body, "and which all consist either in motions brought about by the contraction of the muscular fibres, in sensations produced by the appropriate actions of the nervous

system, or in peculiar affections of certain parts of this system connected with the various intellectual operations." The second and third sections on this head, embrace a succinct description of the heart and its appendages, and the history of the discovery of the circulation. In this place, also, he introduces some interesting observations on the transfusion of blood, and gives a copious reference to cases in which the operation has been practised.—In section fourth, he treats of the mechanism of the heart; and in section fifth, he enters into a full account of the properties and actions of this organ. The sixth section is taken up with the consideration of the action and properties of the blood-vessels; and sections seventh and eighth, with an account of the efficient causes of the circulation, and of the nature, phenomena, and cause, of inflammation. The sixth and last chapter of this volume is devoted to the physical, chemical, and animal properties of the blood. We have not room to enter into a proper analysis of these chapters, but we can recommend them to the attention of the reader, as exhibiting in a clear and methodical manner, a full view of the present state of our knowledge upon the topics which they embrace. In conclusion, we again remark, that as a text book for the teacher, or as a manual for the student, we regard this work as decidedly the best that has yet appeared in the English language. E.

ART. II.—*Observations on the Diseases of Females, which are attended by discharges; illustrated by copper-plates of the diseases, &c.* By CHARLES MANSFIELD CLARKE, Member of the Royal College of Surgeons, Surgeon to the Queen's Lying-in Hospital, and Lecturer on Midwifery in London. Philadelphia, H. C. Carey & I. Lea, 1824. Octavo, p. 222.

WE have not for a long time read a work which has afforded us more information than the present one. It is written in plain and perspicuous language, and embraces a great deal of important matter on a variety of the most interesting diseases that fall under the notice of the medical practition-

er. On the symptomatology and distinctive characters of the sexual diseases which form the subject of this work, the author has presented his readers with much authentic and valuable information. The plates which accompany his descriptions of the diseases, are well executed, and aid very materially in forming a proper conception of the appearance and nature of the structural derangements belonging to these maladies. With these general expressions of our opinion as to the merits of the work, we proceed to give such an analysis of its contents, as will enable our readers, in some degree, to judge for themselves concerning its character and value.

The first chapter contains a short account of the *secretions* of the sexual organs of females, introductory to the consideration of the diseases of these parts. In Chap. II., the author treats of the *Profluvium Vaginale*, or those morbid discharges from the vagina which have been described under the various denominations of fluor-albus, leucorhæa, fluor muliebris, &c. No attempt, he observes, should be made to arrest these discharges when they arise from an inflammatory action of the mucous membrane of the vagina, until the inflammation is subdued; for the discharge itself has a tendency to moderate the inflammatory action upon which it depends, and it would in such cases be "as injurious to restrain the discharge from the parts, as it would be to put an end to the natural salivation of a teething child, whilst the determination of blood to the head continues." There are five varieties of vaginal discharges; the transparent mucous discharge, the white mucous discharge, the watery discharge, the purulent, and the sanguineous. This variety in the character of the discharges, forms the foundation for the classification of the diseases treated of in this work. In the third chapter, the author makes some interesting observations on sexual diseases, and on the necessity and mode of making an examination per vaginam. He strongly urges the necessity of such an

examination in diseases of this kind. "It is quite impossible, in many complaints, to depend on a mere history of the symptoms given by the patient, and daily experience demonstrates the futility, and in some cases the injury, arising to patients from medicines prescribed upon such vague information."

Chap. IV. *On Sympathies.*—A concise reference is here made to the various sympathies of the animal system both in its healthy and morbid state. Besides the stomach, there is, perhaps, no organ that has such an extensive circle of sympathies as the uterus.

Chap. V.—In this chapter the author describes those diseases which are attended *by a mucous discharge from the vagina.* Some of these complaints consist of a displacement of parts: as procidentia of the uterus, bladder, vagina, and inversio uteri.

The uterus often descends very slowly, and many months, or even years, may elapse before it has sunk down very low. Neither the violence nor number of the symptoms are in proportion to the degree of the descent of the uterus; "for when it has sunk but little, the round ligaments are put upon the stretch, and the symptoms, also, which depend upon sympathy, attend the minor cases of the complaint." The most common cause of this complaint, says Dr. C., is the long continued erect posture at an early period after delivery, or after abortion. The author describes the symptoms of the disease minutely. The sympathetic affections are often very distressing; the stomach, especially, suffers greatly from its sympathy with the uterus in this complaint. The appetite is disordered, or entirely lost; flatulence, producing a distressing sense of distention of the bowels, attend; "the spirits flag, every employment becomes irksome, and life itself is considered as scarcely desirable. The diaphragm is sometimes affected with spasm, and hiccough is produced." When the uterus sinks so low as to become an external tumour, the surface of the protruded part ceases to secrete, al-

though whilst within the vagina its secretions were copious. This circumstance explains the fact, that in those cases where the uterus remains within the os externum, there is usually much more general debility than in those where the tumour is external. When a tumour of this kind is returned into the cavity of the vagina, its surface again begins to secrete a mucous fluid. Procidentia uteri is commonly easily relieved when attended to in its early stages. "If a woman having this disease should again become pregnant, and will consent, after her labour, to a confinement of some weeks in a horizontal position, the parts may regain their tone so as to render any artificial assistance unnecessary."

In the cure of this disease it is of importance to give tone to the vagina, by which its diameter will be diminished, and the descent of the tumour prevented: "it is also important to lessen the quantity of the mucous secreted, 1st, because it becomes a cause of weakness, and 2ndly, because the passage of a warm gelatinous fluid over the parts will relax them still more." The employment of cold water, both externally and in the form of injections, will often contribute very considerably to the production of these beneficial changes.

"In procidentia uteri, cold water ought to be applied to the female parts, to the belly, and to the back, by means of a sponge, three or four times a-day; and the water for this purpose should be used as soon as it has been drawn from the spring. The water may be rendered still colder by the addition of ice or salt. Cold water may also be thrown into the vagina by means of a syringe, or a piece of ice may be introduced into the vagina, and suffered to dissolve there. In very slight cases of the disease, when the symptoms are just beginning, and when they are known to proceed from the causes which have been mentioned, they will be removed by attention to these rules, assisted by a horizontal posture. Whenever it is found necessary to inject fluids into the vagina, and important that they should remain there for any time, the operation should be performed when the woman is in a recumbent position; and if a pillow is previously placed under

the hips, in order to raise them a little, the fluid will be less likely to escape." p. 38.

Astringent injections are in general very serviceable. They tend, at once, to diminish the diameter of the vagina, and to lessen the secretions. The author observes, that the different mineral and vegetable articles of this class of remedies are not equally applicable to every case. Where there is much relaxation of the orifices of mucous glands, the sulphate of zinc, nitrate of silver, sulphate of copper, and the nitric and sulphuric acids are much more serviceable than in cases where "tone is required to be given to muscular fibres, as is the case in procidentia." The acetate of lead is seldom useful, unless inflammation of the vagina attends. Alum is one of the best astringents in this complaint. "A mixture of alum, and sulphate of zinc, will sometimes answer better than either of these substances employed alone." Among the vegetable astringents, the oak bark and galls are decidedly the best. A solution of alum, or the sulphates of copper and zinc, in a decoction of oak bark, forms, perhaps, the most useful injection of this kind.

Injections of this kind should be used three or four times daily. In addition to the local treatment, considerable advantage will be derived from the exhibition of tonics internally. Bitters, in combination with astringents, act more beneficially in this disease, than bitters alone. We have been in the habit of employing alum in union with ipecacuanha in cases of this affection; and the effects of this combination have generally been decidedly beneficial in our practice. Six grains of powdered alum, with one and a half, or two grains of ipecacuanha, may be given three times daily. Generally, it keeps the bowels in a soluble state, and we have uniformly found it to diminish the discharge, as well as the procidentia. Dr. Clarke thinks that the cinchona, combining astringency with bitterness, is to be preferred to all other remedies. Where the stomach is much disordered, pure bitters,

however, answer better than bitters and astringents combined. It is of the utmost consequence, in this affection, to attend to the state of the bowels. Too much looseness and too much costiveness are alike injurious.

After all our efforts to relieve the disease by medicinal agents, we are frequently disappointed in effecting a cure. In such cases, the introduction of a pessary into the vagina, becomes necessary, and the relief obtained by supporting the uterus in this way, is, in general, very considerable. An oval or circular pessary will answer very well where the degree of the procidentia is not very great. The diameter of the pessary should not in general be more than two and a half inches, for one of a larger size might do much harm by compressing the urethra and rectum. It is of much importance to remove the pessary occasionally, for the purpose of cleansing it, and it should also be changed from time to time, for a smaller one.

"In consequence of omitting to withdraw the pessary," says Dr. C., "the surface of it has sometimes become coated with gritty matter, which has brought on irritation, inflammation, and ulceration, of the neighbouring parts. Instances too have occurred, where parts of the instrument have been destroyed by a spontaneous change taking place in it, and angular portions of it have been left, which have produced similar bad effects." p. 46.

The author relates the case of a lady seventy-five years of age, who suffered with much pain in the vagina, attended with offensive discharges: "the pain was constant, and was compared to that produced by a sharp instrument." On examination per vaginam, he found that a pessary which had been introduced thirty-five years before, "had lost its original form, being corroded in several places, and that many irregular portions of it were left, which pressed upon the vagina, and had produced ulceration of its internal surface."

In those cases where the uterus has passed out of the cavity of the vagina, it should, if practicable, be returned and re-

tained in the vagina by means of a pessary. The reduction, however, of prolapsed uterus, when it has proceeded to this extent, is in general very difficult, and where there is pain and inflammation of the tumour it must not be attempted at all.

"In almost all the cases in which the degree of the disease is so considerable, every pessary which can be introduced will be forced away by the lightest efforts of the woman: even the globular pessary (which is the best) will not be retained, neither can it be kept in the vagina by any common bandage. But by the following contrivance the globular pessary may be kept in the vagina. In the first place, a pessary is to be chosen of the size which the case requires, and a small slip of brass is to be attached to it by its two ends, leaving a space between the instrument and the centre of the piece of brass. A belt of leather long enough to go round the patients' body, is also to be prepared: to the centre of which behind, a brass wire, as thick as a common quill, is to be attached by a screw. This wire is now to be properly bent; and the pessary being introduced into the vagina, the wire is to be passed between the pessary and the piece of brass attached to the fore part of the circular strap. The reduced parts are by this means supported by a pessary, and this is kept in its place by the unyielding piece of metal." p. 49.

Chap. VIII. *Procidentia vesicæ.*—A prolapsus of the bladder has been frequently confounded with procidentia uteri. Its characteristic symptoms, observes our author, have not been pointed out by any author with which he is acquainted. The majority of the cases of this affection which had come under his notice, were attended with violent coughs. The patient complains of a bearing down; this sensation is, however, not so great as in procidentia uteri. She is more uncomfortable when the bladder contains a considerable portion of urine; for in this case the tumour is much increased in size and weight, and vice versa. "The peculiar symptom which marks this complaint, is a pain referred to the navel, with a sense of tightness there. This pain is the greates

when the bladder contains the largest quantity of urine; and as it parts with its contents, the uneasiness diminishes, till at last, when it is empty, or nearly so, the symptom goes off altogether." On examination per vaginam, a tumour is found in the vagina, situated between the os pubis before, and the uterus behind, which a practitioner can hardly fail to discover is formed by a fluid.

"The distinguishing mark between procidentia vesicæ and procidentia uteri, is the absence of those stomach symptoms which attend the latter; for although the stomach sympathises with the bladder under many states of disease arising from altered structure or disordered actions, it is by no means so frequently affected by disorder of the bladder, as by that of the uterus, and very rarely, if ever, is affected by mere displacement of the bladder. A distinctive mark occurs on examination. In procidentia uteri, an opening is perceptible at the lower part of the tumour, which is not the case in procidentia vesicæ." p. 52.

This complaint may be remedied by the use of a pessary. The hollow pessary of a globular form is the best. Astringent injections, also, are of essential service. The bowels must be kept in a relaxed state, and the urine must never be suffered to accumulate in the bladder. The patient must avoid lifting heavy weights.

Chap. IX. *Procidentia vaginæ.*—Chap. X. *Inversio uteri.*—We pass over these chapters as containing nothing of any particular interest that may not be found in the common works on midwifery.

In Chap. XI. the author considers those mucous discharges from the vagina, which depend on an increased determination of blood to the sexual organs. The circumstances which have an especial tendency to give rise to such local determinations, are hæmorrhoids, ascarides, carcinoma recti, carcinoma uteri, polypus uteri, fleshy tubercle of the uterus, watery tumours arising from the vestibulum, vascular tumour of the orifice of the meatus urinarius, and thickening of the cellular mem-

brane surrounding the urethra throughout its whole extent, accompanied by a varicose state of the vessels of the part.

Each of these affections is treated of under its proper head, at considerable length; but we shall here notice only a few of those which appear to us the most interesting.

Chap. XIII. *Carcinoma recti.*—This disease does not occur frequently. It is always attended with a mucous discharge from the vagina. It has been sometimes mistaken for stricture of the rectum, and the consequent treatment has, of course, been injurious. In carcinoma of the rectum, the pain is greatly increased by the passage of the fæces, and "the pain is such as the patient would feel upon the rough handling of any external tumour of a similar character." In common stricture, on the contrary, the pain is much less severe and acute; being the consequence "merely of a resistance offered to the passage of the contents of the rectum." There is also usually much more constitutional disorder in carcinoma than in stricture. Carcinoma recti is commonly attended with great emaciation, in consequence, chiefly, as the author supposes, of the mesenteric glands having become diseased in a similar manner; although the pain, irritation, and quantity of the vaginal discharge, must contribute to the same effect. No medical treatment, in the opinion of the author, is adequate to the cure of this distressing complaint. The only mode by which a cure can be effected, is excision. Considerable aleviation may, however, be obtained from a proper attention to the diet, which should be light and digestible, and by keeping the bowels in a gently relaxed state. The author says nothing concerning the employment of the narcotic remedies in this affection. We saw a case of this disease some years ago, in which large doses of hyoscyamus afforded unequivocal relief, although the disease, finally, proved fatal.

Carcinoma uteri.—"By carcinoma uteri," says Dr. C., "is meant that disease where there is a tumour near to, or a thickening of the cervix of the uterus, which tumour or

thickening is disposed to ulcerate." The diseases which Dr. Baillie describes under the name of scirrhus uteri, and tubercle of the uterus, are treated of together by Dr. Clarke, under the title of "fleshy tubercle of the uterus."

"The disease (carcinoma) attacks only, in the first instance, the cervix of the uterus. All other tumours, although hard in their texture (scirrhous as they have been called) are of a different character, have different symptoms and terminations. In the dead body they may have some resemblance to carcinoma, but they are never found ulcerated. Inflammation may take place in, or near them, and matter may form; but when the first takes place they suppurate from their centre, and not from their surface: and when they appear to ulcerate upon the surface, it will probably be found that the ulceration is confined to the parts in the immediate neighbourhood, upon which pressure has been made by the tumour itself.

"Two varieties of this disease are to be observed in the early stage. 1.—There is a firm tumour of a rounded form, springing from the surface of the cervix uteri, or imbedded in it, whilst the other parts of the uterus are perfectly healthy, except that its parieties are thickened as the disease advances, and that its cavity becomes larger than that of a healthy unimpregnated uterus. 2.—Instead of a distinct tumour, the whole of the cervix of the uterus becomes larger and harder; and if this thickened part is examined by cutting into it, it puts on the same appearance which a regular carcinomatous tumour possesses. The two cases proceed differently. In addition to the usual symptoms of carcinoma, there will sometimes be found in the first variety of the disease, some mechanical symptoms, depending on the pressure made by the tumour upon the neighbouring parts; which symptoms will be more or less severe, according to the size and situation of the tumour itself. In the second variety of the disease, these symptoms do not exist; because the carcinomatous thickening of the cervix uteri rarely acquires a sufficient size to produce them." p. 79–80.

Symptoms.—The patient has a sense of weight in the vagina, attended with a mucous discharge, which is occasionally tinged with blood, especially when the diet taken has been

stimulating. Pure blood sometimes comes away after active exercise. So long as the discharge is moderate, the disease advances very slowly, and produces not particular inconvenience. The menses become irregular and more profuse. No great inconvenience is, in general, felt from the mechanical pressure of the tumour; for its size is not often so great as to become a source of irritation from this cause. Œdema of the feet and legs commonly attend. There is not often any difficulty in passing the urine, but strangury is almost constantly present. A mucous discharge comes from the bladder. "This latter symptom," says Dr. C., "has often led to a mistaken idea of the nature of the disease, and in some instances the erroneous opinion has been strengthened by another symptom which attends both diseases, viz, *urticaria* or nettlerash." Connected with this disease, there are also frequently symptoms denoting considerable weakness of the digestive organs, such as cardialgia, acidity, flatulency, &c.; and these symptoms are sometimes attended with hysterical symptoms. On examination if the disease be carcinoma, "the cervix of the uterus will either be found thickened, and with a resisting feel resembling that of a gristle; or a distinct tumour will be perceived arising from some part of the cervix uteri, the other parts remaining healthy. In either case, pressure on the diseased part produces pain of a lancinating kind."

Treatment.—As we are not as yet acquainted with any mode of treatment except, perhaps, excision, by which the disease may be radically cured, all that remains for us to do is to palliate its symptoms, and to impede, in some degree, its progress. With this view, it is of much consequence to attend to the state of the general system,—to diminish the mass of the blood where there is plethora,—to keep the bowels in a duly relaxed condition,—to remove congestions of blood in the pelvic viscera, by topical bleedings,—and to direct our patient to use a mild and digestible diet, and to avoid every thing which has a tendency to fatigue and heat the system. No attempt should be made to stop

the discharge, but the parts should be frequently washed with tepid water, to remove the irritating secretions from the vagina. "The heat of the water should be accurately regulated;" if it is too cold it will give too great a check to the secretions; and if too warm, there will be danger of its exciting too much the local vessels. The temperature may vary from 86° to 94° of Fahrenheit, and the water should be injected several times daily. Warm clothing is recommended by the author; 1st, because if there is much blood upon the surface of the body, less will circulate in the interior vessels: and 2ndly, because the chance of inflammatory action being produced by any sudden change of temperature to which the body may be exposed, will be diminished, and the uterus will be less likely to suffer." The urticaria which sometimes attends this disease, is generally increased by a vegetable diet. Dr. C. has not obtained any advantage in the relief of this symptom from external applications; and all those internal remedies which cool and allay irritation, will be found injurious, or at least useless.

It is chiefly important, in the treatment of this disease, to attend to the state of the stomach. The bowels must be kept in a relaxed condition, and some light bitter infusion administered, to which a few grains of soda may be added. The following case is related by the author, in illustration of the importance of attending to the state of the stomach in diseases of the uterus, "which are disposed to become active."

"An unmarried lady, about thirty years of age, had an enlarged uterus, in which, at different times, inflammation had taken place, which was subdued by local bleeding, purging, abstemiousness, and quiet. Some time after she had pursued this plan in the country, she came to the house of her father, in London, and was attacked with cardialgia and urticaria to a very great degree. The skin in many parts of the body was attacked by the latter disease, but especially the face and the lower part of the abdomen. Considerable itching was present, accompanied by irritability of the whole system. The disease of the uterus,

also, which had been in a quiet state during several months, returned with great violence. Local bleeding in some degree lessened the pain: purging with neutral salts increased the urticaria. Nearly all vegetable food was abstained from: she took nothing but soda water and mint tea for beverage, with a little thin dry toast for food. Once in four hours a draught was exhibited, consisting of carbonate of magnesia, compound spirit of ammonia, and an infusion of dill seed. Under this plan all the symptoms subsided, and the uterine disease, at that time, gave no farther alarm. For some months this lady continued well; but having, at supper, eaten freely of fruit pye, and drunk at the same time some sweet wine, the urticaria returned in the night, and with it the pain in the region of the uterus. Some bitter infusion, with rhubarb and volatile alkali, was now given, and soda water for common drink; and again all was put to rights. She now carefully avoids such food as is likely readily to ferment, and watching the first attack of heart-burn, has not for some time been visited by her troublesome complaint." p. 87.

The author has seen no good effects from the use of the rust of iron, in this disease,—a preparation which has been strongly recommended in similar affections situated externally. The muriate of gold has lately been given in this disease, and, as it is alleged, with great advantage.* We have ourselves prescribed it in a case of this kind, with evident advantage, but as the case is still under treatment, we cannot say what degree of relief may be ultimately derived from it. Where the disease has proceeded so far as to become ulcerated, we are informed by an able practitioner of this city, that injections into the vagina of Fowler's solution diluted with water, have been decidedly beneficial.

In the seventeenth chapter, the author treats of the "*Fleshy tubercle of the uterus.*"—This disease is described as consisting of a firm whitish tumour, situated "sometimes upon the surface of the uterus, between the muscular and peritoneal

* Neil.

coat, sometimes projecting into the cavity of the uterus, and occasionally imbedded in its substance." There are frequently several of these tumours, and they are found to vary in size from that of a pea, to that of a child's head. When the tumour is large, the texture is not so firm as it is in such as are small. It seems to be made up of distinct parts, connected by a dense cellular membrane, "the diseased tumour itself being opake, and the connecting membrane more or less transparent." Coloured injections forced into the uterine vessels, do not pass into the tumours. The tumours constituting this disease, are generally situated on the fundus of the uterus, and at a distance from its cervix. These tumours have no tendency to ulcerate, or to enter into suppuration; ulceration and suppuration may, however, occur in the surrounding cellular substance. It is sometimes connected with corroding ulcers of the os uteri, although it does not appear that there is any direct connection between them. There is almost always an increased discharge of mucous from the vagina in this affection.

"The early symptoms are a frequent disposition to make water, and empty the rectum. Cramp in one or both the lower extremities, œdema of one or both feet, are symptoms attending those cases where the sciatic nerve or trunks of the absorbents are pressed upon. When the tumour becomes so large as to fill the cavity of the pelvis, there may be great difficulty in passing the fæces, and a total inability of emptying the bladder. The weight of the tumour, and its pressure upon the parts at the lower aperture of the pelvis, will occasion a sense of bearing down; and this will not be so much relieved by the horizontal posture, as in simple cases of procidentia arising from relaxation. If an examination be made, a hard, large, resisting tumour will be felt; but the os uteri will have undergone no change. It will not gape as in carcinoma, neither will the patient complain of pain when the tumour is pressed upon. The constitution is seldom affected by this disease, and when it is so it is merely from the effects arising out of pressure made by the tumour upon other parts."

Chap. XVIII. *Verrucæ, or warty tumours arising from the vestibulum.*—A mucous discharge from the vagina attends these verrucæ when they are large. They are generally quite insensible; they are sometimes found to grow on the labia, the nymphæ, the vestibulum and the carunculæ myrtiformes. They often arise in parts which were recently the seat of chancres, "but it is not necessary to the formation of these tumours, that the part should have been the seat of any syphilitic complaint, for they may arise in parts which have never been so affected." These excrescences often excite an excessively tormenting irritation in the parts. The tumours may be removed by ligature, or with a pair of scissors; the hæmorrhage is readily restrained by the application of the lunar caustic. They may also sometimes be removed by stimulating applications, "such as solutions of argenti nitras, hydrargyri murias, cupri sulphas, ammoniæ carbonas, &c."

Chap. XIX. *Vascular tumour of the orifice of the meatus urinarius.*—This disease is attended with great pain, and generally occasions much alarm and anxiety to the patient. The tumour arises from the natural projection around the orifice of the urethra; its texture is generally not firm, and it is of a florid colour, resembling arterial blood; its surface has a granulated appearance, and "*it is exquisitely tender to the touch.*" On examining it closely, it presents the appearance of shooting from the inside of the urethra; it is seldom large; "its attachment is so slight, and its form so moveable, that it appears like a detached body lying upon the parts." This tumour, when injured, pours out a florid blood. The author recommends the removal of the tumour by ligature, or by the scissors and caustic. A case of this kind came under our care during the preceding year, in an unmarried woman about forty years of age, which was, at times, attended with the most violent piercing pains in the tumour. After trying a great variety of means ineffectually, the patient was at last completely relieved by the application of the unguentum citrinum.

Chap. XX. A *thickening of the cellular membrane surrounding the urethra throughout its whole extent, accompanied by a varicose state of the vessels of the part.*—Pain and difficulty from the sexual intercourse, sometimes lead to a knowledge of the existence of this disease. "On examination, a bulbous tumour will be found situated behind the pubis," and some degree of pain will be experienced if pressure is made on it. There is always a mucous discharge, probably, both from the vagina and the urethra. When the patient presses down, "the diseased parts come to view, putting on the appearance of a tumour," having large blood-vessels ramifying on its surface. The patient is much harassed by a frequent desire to make water. Warm applications are eventually injurious, although they may afford temporary relief. "The most speedy and effectual mode of relieving the patient is by emptying the vessels, either by puncturing them with a lancet, or by the application of leeches." When the vessels are emptied, the parts should be washed with cold lotions composed of a solution of the acetate of lead; and after a few days, a weak solution of muriate of ammonia, or sulphate of zinc, will be useful. "Pressure is serviceable, and may be made by introducing a piece of wax candle, or a small roll of linen."

Chap. XXI. *On the transparent mucous discharge from the vagina, not accompanied by any alteration of structure of the sexual organs.*—Under this head the author considers the two distinct and very dissimilar diseases of a "*transparent mucous discharge arising from increased action of the vessels*," and "*transparent mucous discharge depending on debility.*" The former of these complaints is apt to occur in plethoric and corpulent women, who lead a sedentary life, and indulge in the pleasures of the table, and "who are generally weak, although they may have the appearance of strength." In cases of this kind, a slow enlargement of the liver usually takes place.

"The vaginal discharge," says Dr. C., "increases in quantity, the fluid of menstruation, also, is secreted more plentifully, and the intervals between the periods are generally shorter than natural: and these symptoms, for the most part, lead the patient to apply for professional advice. Upon inquiry it will be found, that fits of giddiness and of sleepiness have attacked the woman; that there has been pain in the head, perhaps indistinct vision, or a sensation of sparks when they are closed. Many years may elapse before any danger is apprehended; and then all at once the woman may be attacked by a fit of appoplexy, or some great internal hæmorrhage; or she may gradually become weaker and dropsical, and at length die."

The mucous discharge is diminished immediately after each period of menstruation. If the discharge is checked by local astringent applications, without "employing any means of unloading the blood-vessels, the violence of the symptoms generally increases." In examining the bodies of those who have died of this complaint, the author has found the uterus somewhat enlarged; and the liver, in some instances, increased to double its size, and uniformly harder than a healthy liver." The treatment consists in lessening the general and local plethora, both by directly depletory measures, and by an abstemious and cooling regimen,—in restoring the healthy action of the liver,—and, finally, in moderating the vaginal discharge. After the plethoric state of the system has been removed, weak astringent solutions may be thrown into the vagina, in order to moderate the discharge. Whilst the plethora continues, injections of warm water alone should be employed. In cases where the transparent mucous discharge depends on a mere local increased action of the parts, general remedies are seldom necessary. "Leeches, or cupping glasses applied to the lower part of the abdomen, or to the back," together with weak astringent injections, will, in general, answer well in such cases.

As the liver, according to our author, is so often the seat of morbid action in this complaint, we are surprised that he has

not said any thing concerning the use of mercurial remedies. —We have a case of this disease under treatment at this time, in which there is evident hepatic derangement, and in which the blue pill given every evening, in the dose of three grains, and the occasional interposition of a saline laxative, produce manifestly beneficial effects. We are inclined to think that the complaint in question is often wholly dependent on a torpor of the circulation, not only in the hepatic system, but also of the whole portal circle of vessels. It is well known that such a state of the liver and portal circulation has a particular tendency to produce hæmorrhoidal affections, or, in other words, to produce a congestion of blood in the pelvic viscera. It would appear quite probable, therefore, that a similar state, determining a congestion in the uterine vessels, is capable of producing an increased secretion of the natural mucous discharge from the vagina, and that, consequently, the disease under consideration is most commonly symptomatic only of a primary derangement in the hepatic system. Indeed, as we have already said, the author evidently lays too much stress on the discharges attending the complaints which form the subject of his work. These discharges are always to be considered as mere symptoms; and they are by no means always among the most important which belong to this class of maladies.

Chap. XXII. *On the transparent mucous discharge depending upon debility.*—The discharge in this disease varies much both in consistence and quantity. It is usually attended with pain in the back. The author thus describes the concomitant symptoms and consequences of the disease.

"The continued drain from the system increases the original weakness; and the quantity of blood remaining, is by degrees so much reduced, that the surface of the body becomes every day paler, till at length the cutaneous vessels are completely emptied of their contents, and at this time the skin assumes an appearance resembling that of a dead body. The colour of the sebaceous glands of the skin is evident through the cuticle; so that to

the paleness of the skin is superadded an appearance of yellowness, which is not the effect of absorption of bile, for the urine will be found clear and colourless, and the tunica sclerotica of the eye will retain its pearl-coloured appearance. The cellular membrane becomes filled with fluid, and the integuments acquire a doughy look and feel. The legs and feet swell towards night, and in the morning this swelling subsides, and the face becomes puffed; a shortness of breathing succeeds, which is increased by the horizontal posture, and is rendered most distressing when the patient is going up an ascent, or endeavours to read aloud. Violent palpitations of the heart occasionally give the woman great uneasiness; and this symptom sometimes increases to so considerable a degree, that the action of the heart may be heard by a by-stander. During the continuance of these palpitations, the patient becomes very faint, and often considers herself to be dying."

The hands and feet are generally constantly cold; the pulse is weak and quick; dyspeptic symptoms prevail; costiveness almost universally attends the disease; and finally hectic fever, with extreme difficulty of breathing, comes on. Formidable as these symptoms appear, they are, notwithstanding, frequently remediable by a judicious treatment.

Treatment.—It is of the utmost importance in the treatment of this disease, that the patient be placed in a dry and healthy situation. In the damp atmosphere of Holland the disease is said to be very common. "The author has repeatedly seen the complaint attended by the worst symptoms in women who live in damp situations, and in the crowded parts of London, in whom they have quickly disappeared upon a removal to a more healthy spot." Gentle exercise should be recommended. The food must be light and nourishing. Tonics should be administered; it will be best to begin with the light bitters, with a few grains of carbonate of ammonia. After the stomach has acquired some strength, instead of the ammonia, the tincture of muriate of iron will be found very serviceable. A combination of steel, myrrh, and alkali, is a valuable remedy, but it cannot be given with advantage un-

til the stomach has acquired some degree of tone by the previous use of bitters, cinchona, &c. Auxilliary to these curative means, astringent injections are highly useful. A solution of alum, or sulphate of zinc, answers this purpose excellently. Cold sea-bathing is also a valuable aid.

Chap. XXIII. *White mucous discharge.*—The author thus describes this variety of vaginal discharge. "It is opaque and of a perfectly white colour; resembling in consistence a mixture of starch and water, made without heat, or thin cream; it is easily washed from the finger after examination, and it is capable of being diffused through water, rendering it turbid." Occasionally this discharge is found to possess the tenacity of glue, but it is never profuse. Generally there are but few symptoms accompanying the discharge; sometimes, however, says Dr. C., the patient experiences "an uneasy sensation in the back and lower part of the abdomen, which gradually becomes converted into pain, and this pain is increased by whatever calls the parts concerned into action," such as riding or pressure from the expulsion of hard fæces, &c. Irritation of the neck of the bladder, and of the rectum, is a frequent symptom. The constitution seldom suffers in this complaint. On examination, the vagina and external parts are not found to possess a morbid degree of sensibility; but the cervix uteri, when touched, is painful, giving a sensation "which has been compared to that which has been experienced upon the passage of an evacuation from the rectum." The cervix uteri is, however, not altered in structure; there is no thickening or peculiar enlargement, nor ulceration. The author states that he has no hesitation in saying, that wherever the white mucous discharge is present, a tenderness of the cervix uteri will be found to exist on examination. The neck of the uterus is sometimes exceedingly tender to the touch, without the neighbouring parts possessing at the same time an augmented degree of sensibility. In such cases the uterus is generally very low. In some instances, however, the action of the adjoining parts is in a state of morbid irritation.

The author relates an interesting case in point. In some instances the pain in the back and the lower part of the abdomen is at times exceedingly violent. Encysted tumours in the neighbouring parts are sometimes found connected with this disease. The author relates several cases of this kind. Although the constitution does not generally sympathise much with the local irritation, yet the author has met with cases where a great deal of constitutional disturbance was present.

Treatment.—The antiphlogistic plan of cure, as the author observes, is manifestly indicated in this affection. Bleeding, either general or topical, is a primary curative means. The hip-bath also should be used, of the temperature of about 90°, together with fomentations to the back and the lower part of the abdomen, and injections of tepid water into the vagina. A soluble state of the bowels is essential in the treatment of this malady; for this purpose the patient should use, as often as may be necessary, small doses of sulphate of magnesia, or where the habit is languid, Ol. Ricini. Some diaphoretic and anodyne medicine should be exhibited every evening. The author recommends the following composition for this purpose: ℞ Pulv. ipeca. compos. gr. v. P. camphoræ gr. iii. confect. rosar. gr. iii. ℿ. dived. in pul. tres omni nocte sumendi cum hoc haustu. ℞. Succ: Limon: ℥ss. Ammon. carbonat. gr. xv. syrup, cort. aurant ℥j. aq. menth sativ. ℥jss ℿ. fiat haustus. When there is much strangury, opium should be given in large doses. It is not often necessary to confine the patient to bed.

The author says nothing concerning the use of astringent injections, in the treatment of this disease. That injections of this kind will frequently produce injury in these affections, there can be no doubt. It appears to us, however, that our author's apprehensions are much too great in relation to this point; and that in the present variety of vaginal discharge especially, such applications deserve the attention of the practitioner. Judging from our own experience, we should say that very considerable benefit may often be derived in this

complaint from the injection of slightly astringent fluids.—Concomitant with the general treatment already mentioned, we have always directed the use of injections of this kind. A weak decoction of wild cherry-tree bark has appeared to us to answer exceedingly well in the present complaint.

The disease, says the author, which bears the greatest resemblance to the one just noticed, is, *inflammation of the substance of the unimpregnated uterus.* These two affections may, however, be distinguished by the *permanency* of the pain, and by the absence of the white mucous discharge from the vagina, in the latter affection; there is also in this disease an increase of pain produced by pressure above the pubis. Inflammation of the *substance* of the uterus is not of very unfrequent occurrence. Most commonly it is produced by some local violence, and it is sometimes the consequence of marriage. The symptoms, according to our author, are: a constant uneasiness referred to the pelvis, gradually increasing, though seldom becoming intensely severe, but notwithstanding greatly interfering with the comfort of the patient, who complains of pain sometimes at the bottom of the abdomen, and sometimes at the back. Superadded to the permanent pain, violent pains occur by paroxysms with irregular intervals; but these are of short duration." The pulse is not often found to be accelerated or hard, in this complaint; nor is the heat often above the healthy standard. The disease is sometimes of very long continuance, with an increase of the symptoms before each period of menstruation. In the progress of the complaint the catamenia ceases to recur.

Treatment.—General bleeding is seldom of any particular service, unless in cases attended with peritoneal inflammation. Topical bleeding, on the contrary, is highly beneficial. Blood should be abstracted once every ten days, at least. Fomentations, with decoction of anthemis, to which laudanum should be added, are to be repeated twice or thrice daily. The patient should be confined to bed, if the disease prove

obstinate, and antimonials, with a few drops of laudanum, administered. Purging is of essential service.

The author here introduces some interesting observations in relation to the nature and treatment of amenorrhœa. The following remarks on the employment of emmenagogus, are judicious.

"There is a class of medicines (formerly called emmenagogues) which were supposed to be capable of exciting a specific influence upon the uterus; and of these the greater number are either of general or local stimulants. It is evident to common sense, that such medicines cannot be exhibited justifiably in all the variety of cases of obstructed menstruation: even were they capable of effecting what is proposed. On no subject, perhaps, have there been more erroneous notions entertained, or more injurious directions given, than on that of diseased menstruation. Prejudice has occupied the place of science, and a popular nostrum has been exhibited, often without, and sometimes with, the concurrence of the practitioner."

"Cases of suppressed menses having been regarded as arising from debility of the system, or of the blood vessels, it has been too frequent a practice in all cases indiscriminately, to resort to the same supposed means of relief,—to the exhibition of chalybeates, gum resins, irritating purgatives, and cantharides. No diseases admit of being treated more generally. If there are cases of obstructed or suppressed menses, where the fluid is tardily secreted, in consequence of general or local debility, there are many others in which an opposite state of the frame becomes the cause of their production. The reason is obvious: the function of menstruation, like the other functions of the body, is best performed when the system is in health. Now health is not constituted by excess of fullness, or by the performance of violent actions, any more than debility or enfeebled action; consequently, the exhibition of stimulants will not influence this secretion, unless attention be given to the restoration of the general health of the patient, even in cases of debility. Instead, then, of resorting to such measures,—to the employment of the whip and of the spur in such cases, let the morbid peculiarities of the constitution, and the

habits of life of the patient be taken into consideration; let the first be counteracted, the second be improved; let the sanguine have her excess of fullness diminished, let the debilitated have her powers augmented; in short, let the general health be amended, and the functions of health will be restored."

Electricity is recommended by our author as a very useful means of exciting the menstrual secretion. "By means of it," he says, "a great number of cases of amenorrhœa have been cured, when no other means had been successful." It should always be applied in the way of the electric shock; —"sparks are of no service. Several shocks should be passed through the pelvis daily. Electricity, however, can never be used with propriety, until the powers of the system have been restored, and until the general health has been established."

Chap. XXIV. *Watery discharge.*—Under this general head the author considers the three following distinct diseases:—1, Cauliflower excrescence of the os uteri.—2, Hydatids of the uterus.—3, The oozing excrescence of the labia.

1.—*The cauliflower excrescence of the os uteri.*—In this disease the os uteri enlarges; the surface of this enlargement has a granulated feel, and consists of a great number of small projections, resembling the upper surface of the cauliflower. The tumour has but little firmness of structure, and pours out a thin watery fluid, which is characteristic of the disease. It sometimes acquires so large a size as to fill up the whole cavity of the vagina, and to appear externally. If the thin membrane which invests it, is injured in an examination, blood of a florid red appearance is poured out. The excrescence is insensible, no pain being produced by pressing on it. It sometimes arises from a small part of the os uteri, and occasionally its attachment is found to embrace the whole circumference. The increase of the tumour is sometimes very rapid. It appears "to be made up of a congeries of arteries," which ramifying on its surface, pour out a thin aqueous fluid in the most abundant manner.

Symptoms.—The patient at first finds the parts more moist than usual; the discharge increasing, by degrees, so as to become a source of great inconvenience; no pain or fetor, however, attends;—in the progress of the disease, the watery discharge becomes still more and more abundant, mixed occasionally with a little blood, until it is so copious as to require the change of twenty or thirty napkins in the course of a day. The patient becomes weak, pale, and depressed in spirits. The slightest exertion of the body is sometimes sufficient to produce a hæmorrhage from the tumour. Generally after a profuse hæmorrhage, the watery discharge is diminished. In the advanced stages of the disease, the digestive functions become deranged, the abdomen acquires a tympanitic condition, hysterical affections supervene, œdema of the feet, and a puffiness of the eye-lids and face come on. Death, in this disease, is usually the consequence of gradual exhaustion by the watery discharge, and sometimes suddenly by hæmorrhage.

Treatment.—A primary indication in the treatment of this disease, is to diminish the determination of blood to the uterine system, and to lessen the quantity of blood circulating in the vessels of this part. For this purpose local bleeding is manifestly an important means. "But although local bleeding," observes the author, "is so valuable a remedy in many cases, it may, when employed injudiciously, hurry the patient's dissolution. It should not be employed when much œdema of the feet is present, or during the continuance of any symptoms indicating great debility. Neither should bloodletting in any case be carried farther than is just necessary to produce the intended effect; particularly when it is considered that the art is in possession of many other powerful auxiliaries in curbing the progress of this malady." If the patient is strong, and the disease not of very long standing, twelve or fourteen ounces of blood may be abstracted, and this repeated every three or four weeks. Sexual intercourse is to be wholly interdicted. The bowels must be kept in a

relaxed state, so as to prevent straining or pressure on going to stool. This, the author observes, is an important measure in many of the uterine diseases, but in none more important than in the one under consideration. "Fruit taken before breakfast, honey eaten instead of butter, a little manna eaten, with a few blanched almonds, or a tea-spoonful of electuary of cassia taken occasionally, will commonly obviate any inconvenience which may arise from costiveness." Cold water injected into the vagina several times in twenty-four hours, often tends to diminish the tumour. A recumbent posture contributes materially to the comfort and safety of the patient. In this disease very considerable benefit may be derived from the use of astringent injections. In using the syringe, however, great care must be taken lest the tumour be injured, and a hæmorrhage produced. "The common female syringe," says Dr. C., "may be used with less risk, if a handkerchief be previously wrapped round it at the distance of about two inches from its point." The astringents recommended by the author, are, sulphate of zinc, alum, sulphate of copper, galls, and oak bark. In cases where the lining membrane of the vagina is very irritable, a decoction of oak bark is, according to our author, the best astringent for this purpose. The powers of the system must be supported by administering light bitters, muriatic and sulphuric acid. The sulphate of zinc, also, is said, by the author, to be very useful as an internal remedy in this complaint. By the judicious employment of the means that have been mentioned, "nearly every case of the disease," says the author, "may be made tolerable, and perhaps such a change wrought in the size, or the actions of the excrescence, in a few instances, as to remove all the symptoms."

We extract the following case as an example at once of the symptoms and character of the disease, and of the author's mode of treating it.

"A lady, apparently about two and thirty years of age, laboured under a continued profuse watery discharge, mixed occasion-

ally with blood; her constitution appeared greatly weakened, she became very pale and emaciated, and together with a slight degree of swelling of the ankles, she was liable to a great palpitation of the heart, and shortness of breath. As her weakness increased, she sought relief from tonics and a stimulating diet, under which plan the disorder increased. An examination being made, a mass was found to project from the surface of the os uteri, from which a quantity of blood issued when it was pressed by the finger. Syncope took place in consequence of this discharge of blood, although the quantity was very trifling. The tumour had no regular form, and as far as the author could ascertain, the whole of it could have been contained in half a walnut shell. The patient was desired to confine herself strictly to the sofa, to use cold water in a lavish manner to the pelvis, and to throw a strong astringent injection into the vagina; she was directed to give up the use of wine, to live upon farinaceous diet and vegetable jellies, and to loose five ounces of blood from the sacrum by cupping, when she had recovered from the effects of her journey to the author's house.

"Some weeks elapsed before the author again saw the patient; her watery discharge was considerably lessened, and the bloody discharge had entirely ceased: the ankles were no longer swelled, and the general health was improved. The patient was desired to repeat the cupping, and to continue the former system of management. Once in about six weeks or two months, the author has seen the patient, who has followed up the same mode of treatment, occasionally losing some blood from the sacrum by cupping." Two years after the author first saw this patient, she called on him again. She was entirely freed from the watery discharge, and her general health and appearance was so greatly improved, that he hardly knew her. On examination he found the vagina so much contracted by the use of the astringents, that it admitted the finger with difficulty; on carrying the examination farther, no difference could be felt between the os uteri of the patient, and that of a woman in perfect health."

There are cases, however, says the author, in which all medical treatment is unavailing. "Still, even in such in-

stances, the ligature holds out a prospect of relief which has now and then been realized." When the tumour is removed by ligature, cold water should be injected into the vagina, and afterwards a strong decoction of oak bark, in which some alum is dissolved. A solution of nitrate of silver, in the proportion of a grain to an ounce of water, or of sulphate of copper, in the proportion of eighteen grains to twelve ounces, forms an excellent injection in cases of this kind.

The oozing tumour of the Labium.—This is an entirely different affection from the warty tumours of the labia and vestibulum already noticed. The warty tumours are attended with increased *mucous* discharges from the *vagina*, "but in the oozing tumour of the labium," says Dr. C., "the discharge arises from the surface of the tumour, or rather from the interstices in the tumour." The discharge is watery and often very abundant. This tumour projects but little beyond the plane of the surrounding skin, often not more than a line or two, and rarely above one-third of an inch. Its colour is nearly like that of the surrounding skin, and it sometimes extends nearly over the whole surface of the labia. A projection very much resembling this tumour, might be made "by the firm application of a piece of fine netting to an œdematous part during a few seconds, the surface being unequal, consisting of irregular depressions and eminences, from the former of which the fluid oozes."

"In the immediate neighbourhood of the tumour, œdema is occasionally met with, but the tumour itself is not œdematous; soon after the surface of the tumour has been wiped dry, a watery fluid begins to ooze from it, and to form drops, which, having become large, at length run off, and keep the surrounding parts in a state of constant humidity; sometimes soreness and excoriation take place, as upon the upper lip, when the secretion from the nostrils is increased, but the tumour itself is seldom rendered more sensible."

On a superficial examination, says our author, the disease

looks somewhat like that form of erysipelas called shingles; but the projecting parts are solid and contain no fluid. The disease occurs most frequently in fat women; the author has not met with it in young women. The chief inconveniences, according to the author, are, an itching, a preternatural heat in the affected parts, attending with a watery discharge, so copious sometimes, as to annoy and weaken the patient very considerably.

Treatment.—When general debility exists, a nutritious diet, cinchona and the tonic bitters must be administered. Heated rooms and warm seats are to be avoided. External applications often afford considerable relief, but the author has never known a case cured by them. Powdered starch "sprinkled on the parts again and again, so as to form a cake upon them, is a very useful remedy." A horizontal posture should be enjoined during its use. The posture, observes the author, has a considerable influence on the discharge, "even when no application is made to the parts themselves." Local ablutions, with solutions of cupri sulphas, argenti nitras, and decoct. querci, have a considerable tendency to moderate the complaint. Spirits, however, says the author, are decidedly the most effectual of all applications; brandy or arquebusade, and even alcohol, may be employed. In one instance the author has removed the labium at the earnest solicitation of the patient.

The remaining portion of this work is devoted to those diseases which are attended with purulent discharges. These are: Inflammation of the mucous membrane of the vagina, abscess of the vagina, ulceration of the os uteri, corroding ulcer of the os uteri, ulcerated carcinoma of the rectum, and ulcerated carcinoma of the uterus. As we have already drawn out this article to a very considerable length, we shall conclude our account of this valuable work with a short analysis of the chapter "*on the corroding ulcer of the os uteri*," and refer the reader to the work itself, from the perusal of which no one can rise without much useful instruction.

This complaint seldom occurs before the period at which the catamenia naturally cease to recur. The author states that he has not seen an instance of the disease before the age of forty.

Symptoms.—At first there is a sense of heat referred to the upper part of the vagina. Instead of the menstrual discharge, which has ceased, "a yellowish discharge escapes, perhaps trifling in quantity, and now and then mixed with a streak of blood; by degrees the sense of warmth is converted into a glowing heat, affecting the region of the uterus; and it is by no means uncommon with the patients to state that they feel 'as if a hot coal was within them.'"—This sensation of heat and its accompanying yellowish discharge, gradually increases, whilst the ulceration progresses *pari passu*. The countenance becomes pale, and general debility is produced. "Supposing," says the author, "that this disease were carcinomatous ulceration, amongst an assemblage of other symptoms, the most prominent would be the *lancinating* pain, which invariably attends that complaint. In the *corroding* ulcer, lancinating pain forms no part of the symptoms." Intense and acute pain is never present in this affection. When the ulcered os uteri is touched with the finger, the patient complains of no pain; she does not suddenly shrink from pressure, as when carcinomatous ulceration is present." She only complains of a sense of *soreness* in the part. "If an examination be made per vaginam, the breach of surface may be readily distinguished, and the extent of the disease ascertained; but no hardness of the parts will be present, no thickening, no deposit of new matter," as in the carcinomatous ulcer. This disease, says the author, uniformly possesses a fatal tendency. The treatment can only be palliative. Local bleeding, the hip-bath, twice or thrice a-day, of a temperature not exceeding 90°, saline purgatives, an abstemious diet, mild tonics, astringent injections, sulphuric acid exhibited internally, rest and mild diaphoretics, constitute the remediate means recommended by the author in this alarming malady. E.

ART. III.—*Specimen Medicum Inaugurale de Iodio.* Auctore. J. B. STARK. Lugdun, 1823. 8vo. pp. 64.

IODINE has been recently introduced to the notice of the profession as a remedy of very active and valuable powers. The discovery of its medicinal virtues is due to Dr. Coindet of Geneva, who, in 1820, published an account of his experience with it in the treatment of goitre, detailing a number of striking examples of its good effects in this disease. As is usual, however, with new remedies, the statements which have since been published concerning its powers, are exceedingly contradictory. By some it is represented as a safe and almost specific remedy in this singular affection, whilst others condemn it as useless and dangerous. Orfila places it among the caustic poisons. He states that a drachm of it given to several dogs killed them in four or five days, when the œsophagus was tied so as to prevent its being ejected from the stomach;—when the œsophagus was not tied, they threw it up and sustained no material injury from it. In those that died from its effects, the internal surface of the stomach was found covered with pustular ulcerations. Majendie, on the other hand, affirms that he injected a drachm of the tincture of iodine into the veins of several animals; and that he gave it to dogs, without their sustaining any injury from it. It only excited vomiting. He states, moreover, that he saw a child but four years old, swallow a tea-spoonful of the tincture by mistake, without experiencing any dangerous effects from it; and that he himself afterwards took the same quantity without injury, or any other consequence than a disagreeable taste of a few hours continuance. Indeed, the experiments which Orfila made on himself with this substance, show that it may be taken in considerable doses without injurious consequences. When he took it in doses of from one to four grains, he experienced no inconvenience from it. Six grains, however, excited a sensation of heat and constriction of the œsophagus, nausea, salivation, pain in

the stomach, and, finally, bilious vomiting, attended with colic pains. These symptoms subsided on using a few emolient enemata.

According to the observations of Hoffman, Ficinus, and Seiler, iodine acts as a general stimulus upon the system.* It is said to have a tendency to produce irregular determinations of blood to the internal organs,—and particularly to give rise to congestions in the lungs and organs of generation. Exhibited in strong doses, and continued for some time, it has given rise to the following effects: slow pulse, palpitation, dry and frequent cough, insomnolency, emaciation, prostration of strength, tremor, œdema, wasting of the mammary glands, and morbidly increased appetite.

It would appear, therefore, that iodine, though not properly ranked among poisons strictly so called, is nevertheless capable of exciting violent and dangerous effects in the animal economy,—and that consequently, its employment requires much circumspection and cautious management.

Iodine may be employed with nearly the same advantage, both internally and externally. For its internal exhibition it is used in the form of a tincture, made either of the pure iodine or of the hydriotate of pot-ash. Dr. Coindet gives the following formulæ for making the tincture:

℞ Iodü - - - - gr. xlviii.
Spir. vini - - - - ℥i.
m

or,

℞ Hydriodatis potassæ - ℥ss
Iodii - - - - gr. vi.
Aq. distillat. - - ℥i.
m

The dose of these preparations is ten drops three times

* Zeitschrift für Natur und Heilkunde, 2ten. Bandes, 2tes. Heft, p. 263, et seq.

a-day, given in a little sweetened water, and gradually increased, until symptoms of general or local disturbance in the system make their appearance, when it must be discontinued for some time.

The ointment is made according to this prescription:

℞ Hydriodatis potassæ - - ℥ss.
Axungiæ - - - - - ℥iss.
℔. ft. ungt.

With this ointment frictions are to be made on the diseased part, twice daily. A portion about the size of a nutmeg is to be rubbed in at each time.

The employment of iodine as a remedy, has hitherto been almost entirely confined to the treatment of goitre, and other glandular indurations. The good effects of burned sponge in goitre, are now known to depend on the iodine it contains.* When iodine proves effectual, the following changes are observed to take place in the disease. After the medicine has been used about a week, the skin over the tumour becomes more relaxed and soft; the tumour itself soon becomes less tense; it begins to lessen in size, becoming more distinct and soft in its progress, until it finally disappears altogether.

The late German journals contain much testimony in favour of the efficacy of iodine in this disease. Dr. Baupt† gave the iodine-tincture in thirty cases, twenty-eight of which were completely cured without any disagreeable consequences from the action of the medicine. The twenty-ninth patient took four small phials of the tincture, without experiencing any bad effects; but his disease was not in the least improved by it. The thirtieth also took it a long time; the tumour had almost entirely disappeared, when the following symptoms of the injurious effects of the iodine made their appearance:

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* Iodine is found to exist exclusively in the *marine cryptogamous plants*.

† Journal der Practischen Heilkunde. April, 1822.

emaciation, fever, thirst, peculiar alteration of the countenance, diarrhœa, morbid appetite, &c. The warm bath, a milk diet, and opium, soon removed these affections. A boy who had a very firm goitre, together with indurated and enlarged parotid glands, took the iodine, and was perfectly freed from his disease in nine weeks.

Baumes* has divided goitre into two distinct species. The one is a disease of the thyroid gland; (*goitre thyroidien*) the other of the cellular texture surrounding the gland (*goitre cellulaire.*) Formey thinks that iodine is useful only when the thyroid gland itself is the seat of the disease; and that where it is ineffectual, the enlargement and induration is seated in the cellular membrane surrounding the gland.†

Hufeland, in a note to Mr. Formey's paper, published in his journal, observes, that professor Graefe directed his attention to the fact that iodine is of service in goitre only when the tumour is free from pain. Where it is painful, and the arterial system is very irritable, this medicine is not only useless, but generally injurious.

The experience of Dr. I. Hoffman, (Zeitschrift für Natur und Heilkunde) also demonstrates the usefulness of this remedy in the disease in question. Ten children who were affected with goitre, took the iodine, under the direction of the physician. Six of these, who were between the ages of ten and eleven years, took at first five drops of the tincture three times daily. The other four, who were from thirteen to fifteen years of age, took six drops at a dose. After the first eight days, the dose for these last was increased to twenty drops, and that for the six younger patients to eighteen. So long as the tincture was given in doses of from six to ten drops, no perceptible effects were observed from its operation. By the time, however, that the dose was increased to fourteen and fifteen drops, an evident change took place in

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* Traité Elementaire de Nosologie. Tom. II.

† Hufeland's Journal, June, 1822, p. 86.

the disease. The diminution of the tumour went on regularly under the use of the medicine, and six of the patients were entirely cured by it. In two of the remaining four, the tumour was very much lessened in size; in one it became much softer; but in another, not the slightest impression was made on the disease. These four little patients were then put under a second course of the iodine, and in the course of five weeks all of them were entirely cured. Not the slightest injurious consequences were observed to arise from the iodine in these cases.

Formey states that from the fifth of February 1821, to the seventh of February 1822, he treated fifteen goitre patients with the iodine. Five of these did not use the medicine regularly; two, however, were evidently much benefited by it. Of the other ten patients, four were completely cured in the course of from twelve to eighteen weeks, and the remainder were much relieved.

According to Dr. Coindet, frictions with the iodine ointment are quite as effectual in this disease, as the internal exhibition of the tincture. He thinks, also, that when the medicine is thus externally applied, it is much less apt to produce those symptoms of general disorder in the system, which we have mentioned, than when exhibited internally.

The remediate effects of iodine are, however, not confined to goitre. Experience has shown that it is a valuable rememedy in various scrofulous affections. Dr. Coindet cured scrofulous ophthalmia, and glandular indurations with it; and we noticed in the first number of our journal an instance of its good effects in scirrhus uteri. A case of this latter affection is quoted by our author from Hufeland's journal, in which the iodine was of unquestionable service. Several practitioners have mentioned its emmenagogue effects. "I. Hoffman," says Dr. Stark, "gave the iodine to two patients with amenorrhœa; one was not benefited by it; but in the other, who was at the same time affected with chlorosis, a perfect cure was effected. Dr. Wachter has employed the iodine in polypus of the nose with the happiest effect.

"The iodine, says our author, is indicated in persons of phlegmatic and obese habits of body; and hence in indolent and lymphatic indurations. On the contrary, it is to be very cautiously exhibited in irritable and plethoric constitutions, where there is a tendency to congestions, inflammations, and hæmorrhages. A weak and nervous habit, a predisposition to phthisis, pregnancy, asthma, hysteric and hypochondriac affections, and an emaciated state of the body, are circumstances which demand great circumspection in the employment of this remedy. During the use of the iodine, it is recommended to take mucilaginous substances, in order to protect the stomach against its corrosive qualities. The author concludes his interesting little work with an account of eight cases in which this remedy was employed. Out of four cases of goitre, one was cured by the medicine; two appeared to be benefited at first, but they afterwards grew worse, and finally proved fatal. In one case no effect was produced. One case of scrofulous tumours in the neck was cured by this remedy; but another case of this kind was not benefited by it. One of the two remaining cases reported, was polypus of the nose. The medicine was used both internally and applied to the polypus in the form of a linimint through the nostril; the patient got well. The last case related, was scirrhus of the left ovarium; this case was much relieved by the use of the iodine.

It is not often that we meet with good Latin in dissertations of this kind; in the present instance, however, we were pleased to find an unusual degree of correctness and perspicuity of diction. E.

ART. IV.—*Lecture on the State of the Blood-vessels in Fevers.* By C. D. MEIGS, M. D.

WE have seldom witnessed a doctrine more confidently proclaimed, and yet, as we believe, more void of solid founda-

tion, than the one which is advanced in this paper. As the author requests "those who are not bound to consider every doctrine not laid down in the written code as an idle and useless innovation, to examine the one he advances carefully," we feel ourselves invited to the task; and we therefore proceed to offer our reasons for the judgment which we have thus, in the first sentence, ventured to pronounce on its merits.

The doctrine in question alleges that the arteries are *debilitated*, and in a state of *decreased* action, when the pulse is full, bounding, and hard, as in the hot stage of an intermittent; and that, on the contrary, they are more *vigorous* and in a state of *increased* action when the pulse is small, contracted, and feeble, as in the *cold* stage of fevers. This is the full amount of the doctrine,—a doctrine which the author, in the plenitude of his zeal, calls "the waters of Jordan," and in which he invites physicians "to wash seven times, and be cleansed from their leprosy of false doctrine in fever."

The action of a circular fibre, as the author correctly observes, tends to lessen its diameter; and, on the contrary, its relaxation or deficiency of action, must increase its diameter. Hence he infers, that, as in the cold stage of an intermittent, the arteries are small and contracted, they must consequently be in a state of *increased* action, and *vice versa*. "In the cold stage," he says, "the arteries contract with more force than in the hot stage," and hence they are in a state of increased action. Again: "when the arteries act most powerfully, when their calibre is smallest, we shall have a shrinking of the surface of the body, with paleness, coldness, cutis anserina, a small and frequent pulse, &c." That the arteries are in a state of debility and relaxation in fever, may be inferred, he thinks, from the state of the capillaries in inflammation. "I believe," says he, "the truth of a celebrated proposition of Vacca, defended by Dr. Lubbock and Mr. Allen, fully illustrated by Dr. W. Philip, and admitted

by Dr. Thompson,—viz: that in inflammation the capillary vessels are dilated and debilitated."

"I *believe* that fever is the archetype of inflammation,—fever being in the whole system what inflammation is in the capillary vessels."

"I *think* that a dilated and debilitated condition of the capillaries being taken in evidence, and as explanation of the phenomena of inflammation, a similar dilated condition is to be as evidence of an analogous state of arteries in fever,—for fever is the archetype of inflammation, and a certain pathology being admitted in one case, is established in the other." In order to support the correctness of these observations, he endeavours to show that the whole circulating system,—the heart, arteries, capillaries, and veins, are "an unit of tissues;" and that all these component parts enjoy the "same *kind* of vitality; and hence," says he, "if the heart be so excitable, and excited as to resist more than is natural, a full dilatation of its ventricles, the vessels will partake of the same pathological condition generally."

Having given this succinct, but, we trust, correct view of the author's theory, it becomes our business to show its fallacy, and to point out the utter futility of the arguments by which he endeavours to support it.

His first and fundamental position, that, "in the *cold* stage of fever the arteries contract with *more force* than in the *hot* stage," is erroneous, being derived from incorrect views concerning the properties of the arterial tubes. Had he said that in the cold stage of fever the arteries *are more contracted* than in the hot stage, he would have spoken correctly; but the assertion that they *act with more force*, is gratuitous, and wholly incapable of demonstration. It must be recollected that the arteries are not muscular tubes;—that they do not possess the irritability and contractility peculiar to muscular structure;—that, in short, their principle of contraction is totally distinct from that of the heart; being the consequence, not of an excitation of muscular contractility, but of an *elastic*

power, inherent in the arterial structure, by virtue of which, these vessels dilate or contract, according as they are distended by mechanical force. This elasticity is what Bichat calls the "contractility of texture;" the contraction which results from its action "occurs only when there is a want of distention of the arterial parities, that is to say, when these vessels cease to contain the blood which resists their contraction." Thus, if a portion of an artery, included between two ligatures, be punctured, so as to give escape to its contents, it loses half its caliber. And, again, if blood be transfused into the veins of dogs, so as to produce in them an artificial plethora, the arteries become much enlarged.* "These experiments show me satisfactorily," says Bichat, "the cause of a large and small pulse. The artery is more or less large according to the quantity of blood it contains." It is not necessary, he adds, that a stimulant be applied to the arterial texture to cause these vessels to contract; the only thing required is the absence of extension. It is, therefore, obviously erroneous, to say, that an artery contracts with *more force* when its caliber is diminished, as in the cold stage of an intermittent; for if there be any difference in the power with which an elastic substance, such as the arteries, approaches its point of quiescence, it would be more reasonable to suppose that the power is greater in proportion as the distending power forces it from that point; and consequently, greater in the arteries when they are *distended* with blood, than when they contain but a small portion of this fluid, so as to allow the elasticity of structure to approach its point of rest. If from an elastic tube distended with some fluid, a portion of its contents were suffered to escape, the tube would, of course, contract. But would any one say that the tube contracted with *more force,* because it had contracted to a smaller caliber? It is precisely so with regard to the arte-

* Bichat.

ries, in the cold stage of fever, or in any other state in which their diameter is lessened. The argument which Dr. M. draws in favour of his theory from the state of the blood-vessels in inflammation, can avail him nothing. His reasoning on this point may be thus stated: in inflammation the capillaries are debilitated and dilated; but, "I *believe* that fever is the archetype of inflammation;" therefore, fever is attended with a *debilitated* and dilated state of the arteries. We do not exactly know what he means by the postulate, "that fever is the archetype of inflammation." What proof has he for this assertion? He has given us none but his ipse dixit,—he *believes* it, and therefore it is so. The circulating system is not, as our author affirms "an unit of tissues," possessing the "*same kind of vitality*" in all its parts. The texture, properties, and diseases of the capillaries, are essentially distinct from those of the arteries. Their dilatation and contraction depends on a principle very different from that which determines the same changes in the arterial tubes. The arteries dilate or contract, according to the quantity of blood which is forced into them by the heart; their condition, in this respect, is the effect either of mere mechanical distention, or of contraction by elasticity from deficiency of distention. The capillaries, on the contrary, act from an inherent principle of contractility;—they dilate or contract, altogether independent of the heart or arteries. In inflammation, for instance, the capillaries dilate,—either because they are debilitated, or because their organic sensibility (according to the ingenious theory of Bichat) is changed, in consequence of which, they admit more blood than they do in a natural state. In fever, on the contrary, the arteries are enlarged, not by their own action, but by the larger quantity of blood forced into them by the heart, or, as may sometimes happen, by the passage of the blood through the capillary or venous systems being impeded; from which an accumulation of this fluid takes place in the arteries. There is, therefore, no *essential* analogy between the state

of the capillaries in inflammation, and the arteries in fever, so far as relates to their action and state of dilatation. Besides these, there are other considerations, obviously repugnant to our author's assertion, that the state of the arteries in fever, is similar to that of the capillaries in inflammations. In every inflammation the capillaries are dilated, and, if you please, debilitated. If, then, according to the broad position of our author, fever be the archetype of inflammation, ought not the arteries in all fevers to be dilated, or filled with an extraordinary portion of blood? But this is not the case. The volume of the pulse in hectic, and the advanced periods of typhous fevers is almost always below the natural standard.

As Dr. M. maintains that a small or contracted pulse is an evidence of increased vigour and action of the arteries, he is obliged, also, to admit that the arteries are stronger, and act with more force in approaching syncope, typhous fever, inanition, &c.,—an admission which, to say the least of it, offers very unfavourable testimony in behalf of his doctrine. According to his notion, increased action of the heart means nothing else than increased *contraction.* "The heart less irritated,—less in *action,* allows its fibres to be completely distended, and its cavities completely filled;—it takes in its full two ounces and a half of blood, which, *when it is thrown into the vessels, produces a large, round, full pulse, &c.*" Here the author admits that the size of the pulse depends on the quantity of blood forced into the arteries by the heart,—that it is "large, round, full," when much blood is thrown into them, and *vice versa.* With what propriety then, does he maintain that a full pulse indicates *debility* and *decreased* action of the arteries, when their state of dilatation is the effect of mere mechanical distention,—when they are dilated or contracted merely because there is a greater or less distending force applied to them, they being in this respect, merely in a passive and not in an active state. Were it true, as he supposes, that increased *contraction* alone constituted increased *action* of the heart, then indeed we should be obliged

to admit, that a small pulse is indicative of increased action, at least of the heart, if not also of the arteries. The heart, however, is not passive in its dilatations; the act of dilatation is not merely a remission from action, but as much the effect of the active powers of the heart, as its contraction.—"In order," says Bichat, "to judge comparatively of the degree of force of the contraction and dilatation, we can extract two hearts nearly equal in size, from two living animals; place immediately the fingers of one hand into the auricles, or the ventricles of the first, and grasp, with the other hand, the exterior of the second, you will feel that one makes as great an effort in dilating, as the other does in contracting.—This fact, already observed by Pechlin, is so much the more remarkable, *as the effort of dilatation is often greater than that of contraction.** Unquestionably, therefore, an *increased* action of the heart consists both of a more vigorous contraction and dilatation of this organ. When, therefore, the heart is increased, it at once receives and expels a larger quantity of blood than in its ordinary state; and as the arteries are thereby more distended, and of course augmented in volume, a full, round pulse is justly held as an evidence of increased action of the heart and arteries.

Having established, as he conceives, his doctrine, that a small pulse is an indication of strong, and a large and round one, of weak arterial action, he proceeds to apply it to the phenomena of fever, in the following way: The causes of fever produce an increased action of the heart and arteries, manifested by a small, contracted, and feeble pulse; this is the cold stage. But the heart and arteries, in consequence of their decreased action, have their energies reduced. Hence, after the violent arterial action during the cold stage has continued for some time, the heart and arteries become debilitated; the veins, which, during the cold stage, were debili-

* General Anatomy, Vol. III. p. 53.—Hayward's Translation.

tated and turgid with blood, now get the upper-hand, and force their contents into the relaxed and debilitated arteries;—the pulse comes full, round, and hard,—evincing the weak action of the heart and arteries, and this is the hot stage.—"But," says he, "what is the third stage? I answer, that the natural termination of the foregoing condition, is to be looked for in some evacuation, which, by diminishing the quantum of fluids, and removing irritating recrementitial particles retained in the former, may reduce the mass, and momentum, and stimulus. The momentum of the circulation is now very great, for the arterial, capillary, and venous systems and the heart are now equipoise,—the whole system is pervious, being equally free for the passage of the blood, with a heart beating oftener, and throwing out more blood than in a state of health." How is this? The Doctor has told us that during the hot stage the heart and arteries are in a state of debility, acting with diminished energy. If this be the case, how can a reduction of the mass of the blood, and an abstraction of stimulus, restore the lost energy of the arteries, and terminate the hot stage? He may, perhaps, tell us, that if we lessen the quantity of the blood, the arteries will contract; and as he considers increased contraction synonymous with increased action, he may suppose himself in possession of a good sylogysm in favour of his position.

In the above paragraph the author tells us, that in the sweating stage "the *momentum* of the circulation is very great." In this we agree with him perfectly; but how can he reconcile this fact with the principles of his doctrine? In the sweating stage, not only the arteries, but the veins, capillaries, and heart, are more dilated than in a state of health, and therefore, according to him, more debilitated. How, we ask, can the momentum of the blood be increased, when the force which moves it is diminished? Again: In the cold stage of fever we are told, the arteries act with increased energy; in the hot stage, on the contrary, their action is diminished. Now, when a vein is opened in the cold stage,

the blood will hardly flow from the orifice; but when opened in the hot stage, it will rush out in a full and forcible stream. How is it, then, that the momentum of the blood is feeble when the impelling powers are (according to the author) energetic; whilst, on the contrary, it is so great when these powers are, according to him, weak?

After having, in his opinion, fully established his doctrine, our author asks: "What does this doctrine teach me? It teaches me that I should commit a murder by bleeding a patient in a violent attack of chill or ague, because I should, knowingly or voluntarily, increase a disturbance in the circulation, of itself often sufficient to extinguish life." Undoubtedly, bleeding would, in most instances, be very injurious in the cold stage of an ague. In our humble opinion, however, the true tendency of this doctrine is rather to lead to, than to deter from, the adoption of such a practice. If, as this doctrine alleges, the arteries in the chill of an ague be in a state of increased action, and the veins debilitated and distended with a superabundant quantity of blood, would it not appear proper to lessen the inordinate quantity of blood in the veins, in order to restore the balance of the circulation? Is not this, in fact, the practice recommended by many of the ablest physicians of the day, in cases of venous congestion? In the chill of an ague the pulse is small, and the veins are overloaded with blood: these things also exist in the congestive form of fever. Why, therefore, should the same condition of the blood vessels in two different diseases teach the practitioner that in the one he would murder his patient, should he bleed; whilst in the other, he would cure him by this operation? If the doctrine in question were true, it could not lead to partial principles of practice in this respect. The truth, we apprehend, is, that in the congestive form of fever, the power of the heart and arteries is not impaired, or materially weakened, but *oppressed.* By bleeding we take off the oppressing load, and this enables the heart and arteries again to act with more energy. In the cold stage of an ague, how-

ever, the heart is *debilitated*,—its energies are lessened by the morbific cause; the blood not being transmitted in its ordinary quantity and velocity through this organ, accumulates in the venous system. If in this state we were to bleed, we should debilitate the heart still more, and of course increase, instead of diminishing, the morbid condition of the vascular system.

Again, our author says, "this doctrine teaches me not to give brandy and red pepper in pleurisy, nor bark in acute rheumatism." How does his doctrine teach him this? He alleges that a full, round, and strong pulse, is a mark of *debility* and *want* of due *action* in the arterial system. Now in pleurisy the pulse is precisely in this condition. Why not then, if he means to be true to his theory, give brandy and pepper? Are not these articles stimulants? Why, therefore, should he not use them in order to excite the debilitated action of the heart and arteries? Will he tell us that brandy and pepper are sedatives,—that they diminish arterial action? He is, in fact, driven to this conclusion; for as these articles, when taken, produce a "full, round, and hard" pulse, and as, according to his theory, this state of the pulse is indicative of *arterial debility*, they can be nothing else than sedatives!

But we fear we have already wasted too much time on the errors and inconsistencies of this singular theory. To us it appears far from being "the waters of Jordan," in which the errors of false doctrine are to be washed off. We view it rather as an ignis fatuus, which, having caught our author's eyes, has led him into difficulties from which we trust he will extricate himself, as soon as day-light again shines upon him. E.

ART. V.—*An Essay on the Blood, comprehending the chief circumstances which influence its coagulation; the nature of the Buffy Coat; with a concise medical view of the state of the blood in disease; and an account of the powers of a saturated solution of alum as a styptic remedy in hæmorrhage.* By CHARLES SCUDAMORE, M. D. F. R. S., &c., &c. 8vo. pp. 162. London, 1824.

NOTWITHSTANDING the great importance which was formerly attributed to the blood by humoral pathologists, and the warm disputes which have since been maintained respecting its nature,—to say nothing of its actual necessity to the animal economy,—the properties and constitution of that fluid have not, as yet, been satisfactorily determined. The genius of Hunter being perplexed in this research, was compelled to resort to an hypothesis for assistance; and subsequent inquiries have generally not been favoured with better success.

Although the circumstance has not been openly avowed, still it is evident that most of the writers who have handled this subject, have ranged themselves on one or other side of the grand question respecting the vitality of the blood. This has necessarily interfered more or less with the judgment of every experimenter; and the consequence has naturally followed, that the mind has been diverted from the real object of inquiry, to a consideration of far less importance to the interests of pathology.

To these objections the author of the work before us is by no means liable. He has pursued a different course, and afforded another example of the benefits to be derived from the inductive method of cultivating our science. Although he has not exhausted the subject, still he has placed it in a light under which its particular features can hereafter be examined with a much greater prospect of success. By submitting every principle to the test of repeated and carefully performed experiments, and by observing with nice discrimination the various phenomena that are presented by the blood in different situations, he has been enabled to furnish his read-

ers with a body of facts and opinions which appear to us too strong to be resisted. We do not think that we should rate this performance of Dr. Scudamore too highly, were we to assert that it is calculated to raise the character which his celebrated work on gout, &c., had already obtained for him. We shall exert ourselves to procure a republication of it, in this country, for we believe that our brethren have heretofore had less opportunity of informing themselves respecting the actual properties and constitution of the blood, than upon almost any other subject connected with the profession. On the present occasion we shall merely bring forward an exposition of the most interesting facts and principles which have been developed in the work before us, with such general illustrations as may appear useful to our readers.

As preliminary to his more immediate subject, the author has drawn up, from the latest and best authors, a very lucid account of the chemical constitution of the blood. He quotes freely from Brande, Berzelius, Marcet, Bostock, and Thomson, whose opinions in relation to this part of the subject, must be considered as authoritative. The most interesting circumstance which has struck our attention in reading these extracts, is the fact that there is a perpetual variation in the composition of the blood, as regards both the proportion of the different parts into which it spontaneously separates, and the proximate as well as elementary ingredients of these several parts themselves. It is not surprising, therefore, that the analyses of different chemists should differ so much from each other, or that the analysis itself should be involved in so much difficulty that its results can only be considered as approximating to the truth, nor is it unreasonable that in consequence of these circumstances, an opinion should be entertained in favour of the long established doctrine concerning the influence of the blood upon the general health.

In detailing the properties of serum, some interesting observations are made respecting albumen, the fluidity and solubility of which, in water, is supposed to be owing to the soda

with which it is combined. "Liquid albumen is soluble in water, and always contains a notable portion of soda, indicated by its action on vegetable colours." "It is coagulated by heat, acids, and alcohol. It is also instantly coagulated by voltaic electricity; and if two platinum wires, connected with a small battery, be immersed into diluted albumen, it will cause a very rapid coagulation at the negative pole, and scarcely any effect at the positive pole." The action of alcohol and acids in coagulating albumen, is accounted for, according to Mr. Brande, by their affinity for the soda which held it in solution. The galvanic influence, also, by accumulating the soda at the positive pole, leaves the albumen to coagulate at the negative one, in consequence of being deprived of its solvent. Caloric is supposed to effect a kind of decomposition of the liquid albumen. "We might thus consider liquid albumen as a compound of albumen and soda dissolved in water; the effect of heat would then be to transfer the soda to the water, and thus occasion a coagulation; and a *solution of soda* is always found oozing from coagulated serum, and has sometimes been called *serocity;* in time it re-acts upon the coagulum, and dissolves a portion of it."

It is quite a remarkable circumstance that the three most important animal substances,—albumen, fibrin, and gelatin, should differ so greatly in all their sensible properties, and yet be hardly distinguishable from each other in their elementary composition. We do not observe that our author has taken particular notice of this fact, although he has quoted Hatchett to prove that albumen is converted by dilute nitric acid into a substance precisely like gelatine. In one of his extracts from Brande, however, it is asserted that "the *chemical* properties of fibrin are those of albumen." Nearly the same thing is true of several vegetable productions, which are altogether dissimilar to each other in their qualities and appearance, and yet present precisely the same component parts, in almost the same proportion, under a chemical analysis. These circumstances appear to be wholly inexpli-

cable on the commonly received doctrine of atomic combinations, which has probably been extended beyond its legitimate sphere. Perhaps a much slighter difference in the proportion of the various ingredients of different substances, than has generally been imagined, may be sufficient to account for such a diversity of qualities.

On the subject of the *colouring principle* of the blood, some interesting extracts are given from Brande and Thomson. It was formerly supposed that this substance existed in the form of red globules, which were soluble in water. But it has been shown by Dr. Young, that the effect of water is to dissolve the colouring matter only, leaving the globule perfectly colourless, and that in this state the globular particles have the properties of albumen. The colouring matter is supposed to consist in a peculiar animal principle, which adheres to the red globules in a healthy state of the blood, but can be separated from them by various re-agents. Respecting the nature of this principle, different opinions have been formed. It was formerly supposed that it was characterized by the presence of iron. Mr. Brande, however, thinks he has proved that the colour of the blood is not to be referred to iron, and that it is connected with the removal of a portion of carbon and hydrogen from that fluid. In addition to several experiments performed by Mr. Brande, the following extract is adduced to support his views. "It is not a little remarkable that blood is used by the American dyers, together with madder, in the preparations of their finest and most durable reds, and that it has even been found a necessary addition to insure the permanency of the colour. This fact alone may be regarded as demonstrating the non-existence of iron as the colouring principle of the blood, for the compounds of that metal convert the red madder to gray and black."

Dr. Thomson, on the contrary, asserts that "when the colouring matter is incinerated, about one-third per cent. of oxide of iron may be extracted from its ashes. This portion of

iron is a constituent of the colouring matter, and, perhaps, the cause of its red colour. But in what way it is united to the albuminous portion of the colouring matter remains unknown." Berzelius also asserts, that when incinerated, the colouring matter leaves one-eightieth of its weight in ashes, of which about one-half is the oxide combined with a small portion of the sub-phosphate of iron; the remainder being composed of lime, phosphate of lime, carbonic acid, &c. He is of opinion, however, that none of these compound bodies existed in the colouring matter; but merely their bases—iron, phosphorus, calcium, &c.; and that they were formed during the incineration.

It has been ascertained by Thomson, that the albumen of the blood leaves the same quantity of ashes as the colouring matter, and that these ashes contain no traces of iron. This will, no doubt, be considered as a strong argument in favour of the opinion that the colour of the blood depends on the existence of iron in its colouring matter. Still the authorities upon this point are so opposed to each other, that it is extremely difficult to decide in favour of either side of the question in dispute.

Our author has performed several experiments with a view of ascertaining the truth in regard to this subject. He dried both the venous and arterial blood of different persons, and of sheep, at a temperature not exceeding 120°, and submitted it, in a pulverized state, to the influence of a magnet. After the most careful observation, he could not discover that the magnet exercised the smallest influence over any portion of the blood. By digesting the ashes of blood in dilute muriatic acid, however, he was enabled to detect some slight traces of iron on the addition of chemical tests. The solution of ammonia threw down a reddish coloured precipitate. This precipitate was washed with distilled water, and re-dissolved in the least possible quantity of muriatic acid. Being then tested by the prussiate of potash, a deep blue colour appeared, and the addition of tincture of galls produced a purple hue.

He observes, therefore, that the chemical results which he has obtained, would lead him to draw a middle conclusion between the opinions of Brande and Berzelius, as to the quantity of iron; but certainly the position of the former professor, that the colouring principle of the blood is not to be referred to iron, is rendered highly probable, if not completely established."

But the principal object of Dr. Scudamore has been to inquire into the circumstances which influence the *coagulation of the blood.* Notwithstanding the labours of Hunter, Hewson, and others, in this department of physiology, the results which they had described did not appear to him very satisfactory; he has, therefore, not only repeated their experiments with great care, but he has also extended his examination to various other circumstances connected with this interesting process.

"In relation to the time and manner in which the blood coagulates, and also as involving some other phenomena, I shall enter into the following considerations:—Temperature of the blood, as affected by the air of the apartment, or by heat or cold artificially applied. The blood placed in vacuo, and also its entire exclusion, in the first instance, from the atmospheric air. The extrication of carbonic acid gas. Specific gravity; and dilution of the blood. Fast or slow stream in venesection. Rest or agitation of the blood. Kind of vessel in which the blood is received. The presence or absence of the fibrinous coat. The blood as being arterial or venous. The vital principle. Influence of electricity and galvanism applied to the blood. Influence of the following gases: oxygen, hydrogen, carbonic acid, azote. Effect of some chemical agents. The question of the evolution of heat during the coagulation of the blood. On the fibrinous, or buffy-coat of the blood. And, lastly, general conclusions, and medical observations."

The details of his experiments are full and conclusive; but they are so extensive, amounting to ninety-six in number, that

we can only pretend to give a brief summary of their most interesting results.

The first four experiments illustrate the influence of temperature in a very satisfactory manner. Equal portions of blood were drawn at the same time, from the same individual, and exposed to different degrees of heat. The result uniformly was, that the process of coagulation became accelerated by an increase, and retarded by a diminution of temperature. Mr. Hewson's assertion, however, that the disposition to coagulate might be entirely taken off by a long exposure to cold, did not prove to be correct. Under the influence of a frigorific mixture, the coagulation of a portion of blood could only be retarded till the expiration of one hour and a quarter, when it began to separate into its constituent parts. The 5th experiment was performed to determine the rate of coagulation *in vacuo*. Of two equal portions of blood, drawn at the same instant, one was placed under an air-pump, and the other exposed in the apartment. "The blood in each cup was 81°. Immediately on the exhaustion being made, the thermometer fell to 80°; in a minute and a half, to 79.5°." At the end of six minutes it had fallen to 76°. The cup exposed to the air of the apartment, on the other hand, was reduced only half a degree in five minutes, and at the end of six minutes it rose one-tenth of a degree, almost to its original temperature. At six minutes and a half he removed the blood from the air-pump, and found it more coagulated than the other portion; it was also considerably darker in colour.

Experiment 6th was a repetition of the former, with a still more decisive result. The temperature of the blood when placed under the air-pump, was 84°. In half a minute after the exhaustion, it fell to 80°; and in five minutes to 75°. The other portion of the same blood, which was exposed to the air of the apartment, lost only one degree and a half of its temperature in the same period of time. "The blood being now removed from the air-pump, was found much more

coagulated than the other portion; it was free from air vesicles on the surface, the other abounding with them. It was almost black in colour, the other having the usual appearance."

The cause of the sudden reduction of temperature under the air-pump, is sufficiently obvious. The pressure of the atmosphere being removed, evaporation must take place more rapidly from the surface of the blood, and, of course, a greater quantity of caloric must escape in union with the vapour. But the cause of the accelerated coagulation of blood under these circumstances, is not so easily explained. In the four first experiments, it was proved that a simple diminution of temperature retards this process. What, therefore, can constitute the difference in the influence of a low temperature upon blood exposed to the air, and upon the same fluid when placed *in vacuo?* According to Mr. Hunter, the presence of air, by stimulating the blood, ought to excite coagulation. Mr. Hewson, also, in his inquiry, evidently attributes coagulation to the influence of the same agent. But it is proved by these experiments, that the blood undergoes a change in its condition more rapidly under a vacuum, than when placed in full communication with the atmosphere.

To elucidate still further the influence of air and of temperature, the four succeeding experiments were instituted. In the first of these, two bottles, fitted with ground stoppers, were filled with different portions of the same blood. In one bottle the stopper was instantly inserted; the other bottle was left open. In five minutes the blood in the open bottle appeared to be coagulated; but in the other bottle it was nearly fluid. The temperature in each bottle continued at 82°. Here was a result contradicting that obtained in the experiment with the air-pump. The blood excluded from the atmosphere coagulated most slowly, the temperature of the two portions being the same. In experiment 8th, 9th, and 10th, these trials were repeated in various ways, all of which furnished the same fact with still stronger confirmation.—In reviewing the subject as thus far displayed, the author was

staggered with the following perplexities. 1.—Low temperature delays the coagulation of the blood, as is shown in the first experiments. 2.—At a reduced temperature, *in vacuo*, coagulation is hastened. Experiment 5th, and 6th. 3.—Atmospherical air being simply excluded in the stopped bottle, the temperature not reduced, coagulation is retarded. Experiment 7th. 4.—All communication with atmospherical air being prevented, the temperature not reduced, coagulation is much retarded. Experiment 8th, 9th, and 10th.

In order to reconcile these apparent contradictions in the results of his experiments, the author was driven to the supposition that "the more or less gradual extrication of the carbonic acid belonging to the blood, was the chief circumstance having an influence on the period of time in which the coagulation takes place." To put this hypothesis to the test, he performed nineteen experiments, in which, by means of lime-water, he was enabled to detect the carbonic acid as it was evolved from the blood. From these experiments it appears to be satisfactorily demonstrated, that whatever circumstances hasten the evolution of this gas, promote coagulation; and, *vice versa*, that the evolution of the carbonic acid takes place most freely when the blood is coagulating, and ceases when that process is completed.

This opinion, which we believe to be original with Dr. Scudamore, harmonizes with all the facts deduced from the previous experiments, and explains away every appearance of contradiction between them. Thus, in the first class of experiments, the low temperature retarded coagulation in consequence of its preventing the extrication of the fixed air: in the second class, the removal of the pressure of the atmosphere was followed by a sudden extrication of the gas, which, of course, accelerated the coagulation, notwithstanding the diminution of temperature: and, thirdly, when the blood was placed in a bottle, the confinement of the gas, as a necessary consequence, retarded the same process.

That carbonic acid gas is evolved from the blood during

the process of coagulation, was fully demonstrated by Sir Everard Home, in the Philosophical Transactions for the year 1818. He placed fresh drawn blood under the receiver of an air-pump, and found that a quantity of gas sufficient to produce an appearance of effervescence, escaped during the action of the machine. He ascertained that this was the carbonic acid gas; and he asserted that it was met with in the same proportions in arterial and venous blood; and that two cubic inches were extricated from every ounce of blood. He did not, however, draw any inference from these observations, in regard to the time of coagulation, as connected with the extrication of the gas. The credit of this suggestion, as far as we can discover from what has been written on the subject, belongs exclusively to Dr. Scudamore.

Our author does not coincide with Sir Everard, in the opinion that two cubic inches of gas can be extricated from every ounce of blood. He performed several experiments with Mr. Garden, in most of which only about forty-five-hundredths of a cubic inch of gas could be detected in six ounces of blood. The proportion, however, varied considerably in blood drawn from different subjects. The largest quantity which, in any instance, he was able to obtain, was "rather less than half a cubic inch from six ounces of blood, a result remarkably in opposition to that quoted by Sir Everard Home."

In one experiment blood was drawn from a young man in health, before dinner, and again two hours after; and it is worthy of observation, that the quantity last taken yielded double the proportion of carbonic acid to that obtained from the blood drawn before the meal.

To ascertain the specific gravity, and dilution of the blood, he performed a great number of experiments, from which the following are his conclusions:

"Blood possessing the highest specific gravity, coagulates the most quickly, and the best specimens of such blood are obtained from persons in health, and of a robust constitution. In blood

of this description, there is the largest proportion of red particles, which are the heaviest parts of the mass. Blood which gives the fibrinous coat in a great degree, has a lower specific gravity than healthy blood, and is slowest in coagulation.—Fibrin is lighter than the red particles, and also lighter than serum. In the two most extreme examples which I have met with, the blood of a robust female in health, was 1.059, and coagulated in four minutes. That of a man labouring under pneumonia, and who was bled for the fifth time, was 1.039, and did not coagulate completely till twenty minutes. I must, however, observe, that moderately sizy blood drawn from a person before he becomes weakened, has not commonly a very low specific gravity."

His inquiries into the comparative specific gravity of different kinds of blood, resulted in a conviction that both the temperature and specific gravity of the venous was somewhat less than those of the arterial blood.

Our author's experience in regard to the influence of the rapidity of the stream in blood-letting, corresponds with that of all other practitioners. The slower the stream, the more rapid is the coagulation; and *vice versa.* Blood drawn from a person about to faint, coagulates very rapidly; and the same fluid passing from the vessels of a dying animal, is almost instantly solid. The author endeavours to explain these facts, by stating "that the blood passing down the arm in a slow manner, parts with its carbonic acid more readily and more completely."

He has also proved what every physician already knows, that agitation of fresh drawn blood favours its coagulation. Mr. Hunter would attribute this effect to the stimulus of motion upon the living principle of the blood, and we believe that most physiologists have been satisfied with his explanation. Dr. Scudamore, however, asserts, that agitation accelerates coagulation merely because it favours the escape of the fixed air from the blood. We will not, for the present, undertake to decide this point, for the doctrine concerning the vitality of the blood, must previously be dis-

cussed, and if that be decided in the negative, every explanation connected with it must of course fall to the ground.

If blood, immediately after being received in a basin, be stirred briskly with a stick, the fibrin will be separated from the red particles, and adhere in a twisted mass to the stick. The ordinary mode of coagulation is then prevented, the coloured portion in the basin remaining perfectly fluid. This he regards as a simple and familiar proof that the solidification of the fibrin is the essential cause of the coagulation of the crassamentum of the blood.

As our author observes, the time in which the coagulation of the blood is effected, differs very much according to the kind of vessel into which it is received. He put equal quantities of blood into a tea-cup, a saucer, and an open-mouthed phial. In three minutes he could raise a thread of fibrin from the phial; not until five minutes in the same degree from the cup, and not till seven from the saucer. At the end of nine minutes the temperature of the blood in the phial was 75°, in the cup 70°, in the saucer 60°. Here the difference in the time of coagulating was evidently owing to the difference of temperature. The vessel which retained heat the longest, solidified the blood most rapidly. He repeated this experiment with different vessels, and found the coagulation most delayed when the blood was received in a pewter porringer, from the obvious circumstance that such a vessel was a good conductor of heat.

The form of the vessel has no influence on the process of coagulation, except so far as it favours the escape or retention of heat. He poured equal quantities of fresh drawn blood into a saucer and a tea-cup, and kept both these vessels at the same temperature by immersing them in a basin of hot water. The blood in each coagulated exactly and equally in four minutes.

Although our author has not mentioned the circumstance particularly, he no doubt wishes to have it all along understood that heat affects coagulation chiefly by means of the influence which it exerts upon the state of the carbonic acid.

This gas always combines with water in an inverse proportion to its temperature; and it is probable that the same thing takes place in regard to the blood. It must be driven off sooner by heat, and retained longer by cold.

There is reason to believe, however, that heat exercises some immediate influence over coagulation, independently of the intervention of fixed air. By elevating the temperature of blood very much, the fibrin is rendered almost immediately solid. This closely resembles the coagulation of albumen by heat; but it is effected at a lower degree of temperature. In a specimen of healthy blood, it required only 120° to solidify the fibrin, while the serum did not begin to coagulate till the temperature was raised to 140°.—In blood of a low specific gravity, taken after the patient had been bled several times, "the same degree of effects required a heat of 130° for the fibrin, and of 160° for the serum." The temperature required, both to coagulate the serum, and to render the fibrin almost immediately solid, appears, therefore, to vary according to the density of the blood.

The following passage is so interesting, that we extract it entire for the benefit of our readers:

"In attempting to explain the coagulation of the blood, I should be induced to say that it depends on a new condition of the fibrin, which can only exist fluid in a state of intimate mixture with red particles, serum, and carbonic acid gas. Except in circulation, or in living vessels, the fibrin will not maintain an union with the serum, although it does continue blended with the red particles, either when extravasated in the living body, or when taken from the vessels. It is therefore the property of the fibrin to become solid when separated from the several principles just stated, constituting in union the mass which we call blood."

He asserts that when the fibrin is in great excess, it is less dense than in healthy blood, and takes the solid form more slowly. This affords a good foundation for the commonly

received opinion respecting the cause of the sizy appearance of blood. Both the want of density in the fibrin, and the slowness of coagulation, favour the subsidence of the red globules, on which the appearance alluded to unquestionably depends.

According to his experiments, it is in inflammation of the fibrinous textures that the greatest quantity of fibrin is found in the blood. He was therefore led to conceive that, under such circumstances, the fibrin, instead of being distributed to the fibrous textures, as usual in health, remains in excess in the blood. The textures in question would, as he thinks, be injured by receiving their usual supply of fibrin, while in a state of disease.

Upon the subject of the proportion of fibrin to the whole quantity of blood, a large number of interesting and instructive experiments are detailed. The result appears to have been that only from two and a-half to four and a-half grains of dried fibrin could be obtained from one thousand grains of the clot of healthy blood; while from ten to thirteen and a-half grains of the same substance could be extracted from one thousand grains of the clot of blood taken from patients in a state of active inflammation. It is also proved very clearly that the proportion of fibrin in different cups of blood drawn at the same bleeding, varies exceedingly. Sometimes there is a difference of more than one-half between the contents of two cups. For example, he found, in one instance of sizy blood, the proportion in the first cup for one thousand grains of clot, twelve grains; and in the last cup not quite six grains. This he explains by the supposition that in the space of time occupied by the venesection, the state of the circulation undergoes a change, so that the capillaries at once make a different distribution of the fibrin; "resuming, in a great measure, their ordinary economy, and conveying it to the fibrous textures, instead of returning it in unnatural excess to the venous circulation."

By watching the progress of coagulation in a phial, he as-

certained that healthy blood coagulates first in the middle, and afterwards at the top and bottom. His explanation is that the middle keeps warmest, and of course parts with its carbonic acid most readily. But the order of coagulation in sizy blood is different. It commences towards the bottom of the vessel; because the fibrin is there in excess, and that which is blended with the red particles, concretes much more readily than that which is mixed with the serum.

His experiments to ascertain the difference in the coagulation of arterial and venous blood were attended with opposite results. Sometimes one, and at other times the other, kind of blood would coagulate soonest. He concluded, therefore, that the only circumstance which has an influence on the coagulation of these two kinds of blood when taken from the same subject, "is the slow or fast stream, which causes the carbonic acid to pass off more or less freely." The temperature of the arterial blood, however, was always a little higher than that of the venous.

To decide the correctness of Mr. Hunter's favourite doctrine respecting the vitality of the blood, our author has performed a great number of exceedingly curious and decisive experiments. After what has already been stated, we hardly need inform our readers that he has come to the conclusion that the changes which the blood undergoes, when drawn from its vessels, are entirely of a chemical nature. By the greater or less proportion and density of fibrin, or by the influence of temperature and the various other circumstances which accelerate or retard the evolution of carbonic acid, he undertakes to explain every appearance of change in the condition of coagulating blood. He has refuted most of the positions of Mr. Hunter with great ingenuity; but, as our limits will not permit us to enter freely into this subject, we must content ourselves with noticing the most prominent circumstances connected with it.

One of Mr. Hunter's advocates, Mr. Thackarah, has asserted that "the coagulation of the blood is considerably de-

layed if received in a vessel recently insulated, or recently removed from a living animal;" and this has been insisted on as a strong argument in favour of the doctrine under consideration. The results of several experiments performed by our author, do not, however, agree with this assertion. He collected a quantity of blood, drawn from a young man in health, in a portion of the small intestine of a rabbit killed on the previous day, and another quantity in a similar portion of intestine from a rabbit killed at the moment of the experiment. The blood received in the intestine of the newly killed animal, gave a small coagulum in eight minutes and a-half, and was rather more advanced than the blood received in the other intestine, which he thought might be attributed to the higher temperature of the intestine just taken warm from the body. Each being examined at the end of fourteen minutes and a-half, he found that the intestine from the newly killed animal contained much the largest quantity of coagulum.

Several other similar experiments, performed with the jugular veins of animals, gave the same results, with this difference, however, that the blood coagulated sooner in the veins than in the intestines, under the same temperature. He explains this difference by observing that the heat and carbonic acid of the blood would be transmitted soonest through the thin parieties of the veins. Such circumstances as these, which have been neglected by other experimenters, have probably given origin to numerous misconceptions on the part of authors.

By intercepting a quantity of blood between two ligatures applied around the jugular vein of a living horse, however, he found that no coagulation had taken place at the end of an hour and three quarters; although the same blood coagulated in five minutes after being drawn into a cup. This corresponds with Mr. Hunter's observations upon blood drawn by leeches, and upon the same fluid when accidentally effused into the cavity of the *tunica vaginalis testis.* But this only

proves that while in living vessels the blood is not disposed to coagulate, and the real explanation may be that in such a situation it is not able to part with its carbonic acid.

One of the arguments upon which Mr. Hunter chiefly relied for the support of his opinion, is drawn from the analogies which he supposed to exist between the coagulation of blood and the contraction of muscular fibre. By cutting out pieces of muscle from an animal recently killed, and placing them in basins of water heated to different degrees of temperature, he found that the force and rapidity of contraction was in direct proportion to the heat of the water. To this he compared the apparently similar influence of heat over the coagulation of the blood; and from the comparison he drew as bold a conclusion as any analogy could well justify. Our author, however, has completely destroyed the force of this comparison by proving, as we have already stated, that heat exercises no direct effect upon the natural coagulation of the blood, independent of the extrication of its carbonic acid.

Another analogy upon which Mr. Hunter laid much stress, is that which he believed to occur between the state of the blood, and of the muscles after death. He asserted that in all cases of sudden death, produced by lightning, by blows on the stomach, hard running, &c., the blood would not coagulate, at the same time that the muscles could not stiffen. But Dr. Scudamore's experiments do not corroborate this idea. He killed small dogs and rabbits instantaneously by sudden discharges from a powerful electrical battery, and by large doses of prussic acid; and in every case he found the blood more or less coagulated after death. He insinuates that Mr. Hunter might have been misled by observing "the flow of serum mixed with red globules, giving the appearance of blood not coagulated, when, on closer examination, he would have found the fibrin detached."

Our author performed several experiments to illustrate the influence of electricity and galvanism upon the blood, when out of the body; and found that its coagulation was promoted

by them, at the same time that its temperature was considerably elevated. The electric fluid did not alter the natural arrangement of the blood in coagulating, but the galvanic action produced instantaneous decomposition. These effects he conceives to be altogether chemical, depending on the direct power of imparting heat which belongs to those peculiar agents. They do not, however, particularly affect the doctrine of Mr. Hunter, inasmuch as he never attended to them in his reasoning upon this subject.

But the most direct refutation of the idea above alluded to, respecting an analogy between the contraction of muscle and the coagulation of blood, can be drawn from this experiment of our author. Experiment LXXII.—"By the addition of extract of *Belladonna*, the fibrin formed into a firm clot."

Belladonna is, perhaps, the most certain counter-agent to muscular contraction in the whole materia medica. It produces relaxation in every muscle to which it can be applied. Of late it has been employed to dilate the os uteri, in parturition; and we all know its use in enlarging the pupil for operations on cataract. As, therefore, our author appears to have proved that the same substance exerts an opposite influence upon the coagulating lymph of the blood, he must be allowed to draw the conclusion that there is less analogy between the two processes compared together by Mr. Hunter, than that illustrious physiologist imagined.

On the question whether heat is evolved during the coagulation of the blood, much interest has been excited.—Whenever inanimate fluids become solid, they always give out some portion of their latent heat; on the contrary, the contraction of muscular fibres, although it renders them more solid, does not appear to elevate their temperature. Mr. Hunter, therefore, conceived it important to his theory to decide this question in the negative; and he made no difficulty in concluding, from an experiment which he performed on a turtle, that the blood does not give out heat during its coagulation. Our author, however, has formed an opposite opinion

from the details of Mr. Hunter's own experiment. At the time of performing it, the temperature of the atmosphere was 65°; that of the turtle, as indicated by a thermometer in the anus, 64°; "and that of the blood while flowing. 65°; when collected it was 66°, but it fell to 65° while coagulating, which it did very slowly, and when coagulated it was still 65°."—In commenting upon this experiment, Dr. S. very properly observes, that "the permanency with which the blood maintained the temperature of 65°, is to be attributed to the temperature of the atmosphere; but how is it to be explained that the elevation to 66° should have been attained, if not derived from a change of capacity in the blood itself, while coagulating?" But the opinion of Mr. Hunter, now under consideration, is refuted more satisfactorily by the results of nine different experiments performed by our author under much less unexceptionable circumstances. The blood of warm-blooded animals, in which the temperature is considerably above that of the atmosphere, was selected; and the fact that its coagulation does elevate its temperature, was repeatedly demonstrated. In all the instances wherein the heat was not actually increased by coagulation, the temperature of the blood was maintained for a considerable length of time, notwithstanding the comparative coldness of the surrounding atmosphere. This our author justly considers as a proof of the same circumstance; for he found that a mixture of starch and water, of the same density and temperature with fresh drawn blood, cooled much more rapidly under the atmospheric influence. The doctrine of the vitality of the blood is therefore assailed, instead of being supported by the condition of the temperature which attends its coagulation.

Some interesting experiments were performed by Dr. S., for the purpose of ascertaining the influence of the gases, and various other chemical agents upon the blood. The results were,—

1st,—That oxygen elevated the temperature of the blood considerably, and accelerated its coagulation.

2d,—Hydrogen diminished the temperature slightly, and retarded coagulation; (this experiment, however, was not, in our opinion, satisfactory, for only one of the portions of blood was placed under the air-pump.)

3d,—Carbonic acid diminished the temperature very much, and retarded coagulation; (this experiment is liable to the same objection as the last.)

4th,—Azote gave to the coagulum a dark colour and loose texture; its effect upon the temperature and coagulation were not distinctly marked.

5th,—A saturated solution of sulphat of soda, added immediately to fresh drawn blood, did not altogether prevent coagulation; a weaker solution rendered it a soft uniform jelly, of a florid red colour.

6th,—A saturated solution of sulphat of potash produced a weak uniform coagulation; the colour florid red.

7th,—A saturated solution of borax produced the same appearances.

8th,—A saturated solution of tartrate of soda, prevents coagulation, but the blood thickens; and if a weaker solution be used, coagulation, of a soft jelly-like consistence, takes place.

9th,—A saturated solution of common salt keeps the blood fluid, and of a dark colour; with a weak solution it coagulates loosely.

10th,—Muriate of ammonia produced the same appearances.

11th,—A saturated solution of nitrate of potash keeps the blood fluid; a weaker solution allows of loose coagulation; the colour florid red.

12th,—Diluted muriatic acid, and diluted sulphuric acid, each equally produced a dark pitch-like colour of the blood.

13th,—The medicinal prussic acid did not appear to delay coagulation.

14th,—By the addition of the extract of Belladonna, the fibrin formed into a firm clot.

15th,—The liquor potassæ, in the proportion of twenty minims to the ounce of blood, rendered the blood permanently fluid; the alkali, therefore, appears to act as a solvent of the fibrin.

16th,—Five grains of the sulphat of zinc, dissolved in one ounce of distilled water, produced a thickening of the blood, but not coagulation.

17th,—Sulphat of copper, in the same proportion, produced rather more effect;—a more dense precipitation.

18th,—Sulphat of alum, in the same proportion, produced rather less effect than the zinc.

19th,—Nitric acid, diluted with seven parts of water, produced a very dense precipitation; the blood remained dark in colour.

20th,—A saturated solution of the three salts last mentioned, was added to fresh drawn blood. The solution of alum instantly produced very dense and tenacious coagula; but the other solutions a grumous thickness of the blood. The sulphat of copper had more effect than the zinc.

In commenting upon these experiments, the author observes that "the effect of oxygen gas in elevating the temperature of the blood, very naturally leads us to the theory of animal heat as depending on the combination of oxygen with the blood in the lungs. If the blood out of the body receive sensible heat from its combination with oxygen, may we not consider that the same effect takes place in the lungs?" &c. The experiments of Dr. Davy, published in the Phil. Trans. for 1814, appear to have disproved the captivating hypothesis of Crawford, respecting the production of animal heat; and our author cannot, therefore, be reprehended for thus attempting to advocate the exploded doctrine of Black. The more rapid coagulation of the blood under the influence of oxygen, was, no doubt, owing to the disengagement of carbonic acid, produced by the elevation of temperature; and the slower coagulation under hydrogen and carbonic acid might have proceeded from

the absence of atmospheric air, the oxygen of which is calculated to generate some degree of heat in the blood.

The effects of the different chemical agents on fresh drawn blood, are thus compared with each other:

"The sulphuric acid itself, diluted as the pharmacopœia directs, blackens the blood; but its combinations with alkalies produce a florid red colour. The muriatic acid, free and combined, very much darkens the colour of the blood. The nitric acid, in a free state, caused the blood to remain of its usual dark colour; but combined with alkali, produced a vivid red appearance. It appears that saturated solutions of several of the salts have the property of suspending the coagulation of the blood; but when the necessary quantity of water is added, the coagulating process takes place more or less perfectly." "The trials with the other salts were made in reference to their styptic quality, as depending on their chemical union with the fibrin, so as to form a coagulum."

In discussing the phenomena which attend the extrication of carbonic acid from the blood, our author controverts, very successfully, as we think, the opinions of Sir Everard Home, in relation to the organization of the coagulum. He conceives that the quantity of carbonic acid gas contained in the blood, is too small to form such numerous air channels as Sir Everard imagined that he could discover; and he suggests that the particles of the gas would not evolve themselves in straight or curved lines, but in globules,—such as could assist but little in the subsequent development of blood-vessels.

In summing up the results of his experiments on the *carbonic acid* of the blood, he asserts that he has proved that it not only varies remarkably in the blood of different individuals, but also that it varies in the blood of the same individual at different times of the day. He has frequently heard patients complain of a peculiar sensation of fullness of the veins, as if they were distended with air. When such a case is urgent, not only the veins, but the skin also gives the sensa-

tion of tightness and distention. It appeared to him highly probable, that, under these circumstances, there was an excess of carbonic acid in the blood. He recommends in such cases, to prohibit the use of all liquors which contain fixed air; and says, "we may extend this prohibition to persons suffering from fullness of habit, or any condition of the vessels in which it is of importance to counteract congestion of blood, or distension of the blood-vessels in any part of the system." Dr. Baillie, in attempting to account for a singular case of spontaneous emphysema, was driven to the supposition that the air must have been generated in the blood-vessels themselves, and thrown out in the form of a secreted fluid into the cellular membrane. If he had been aware of the fact, which has since been substantiated by our author, he would no doubt have considered his theory much more complete.

The following practical observations are so judicious, that we extract them at full length:

"In the examination of the blood in venesection, the practitioner must direct his attention to several circumstances, amongst which the following may be mentioned as the principal: the kind of orifice; the stream in which the blood flows; the state of the pulse during the bleeding; the apparent density, colour, and other appearances of the blood on its being received into the basin; the time in which it coagulates; the external appearance of the coagulated blood; the texture of the coagulum; the comparative conditions of the several portions of blood in different cups; the appearance and quality of the serum.

"The mode of coagulation is materially influenced by the kind of stream, which of course depends chiefly on the orifice made in venesection. When it flows slowly, the coagulation takes place so soon, that the fibrin unites itself with the red particles very uniformly, and therefore does not present any appearance of the buffy coat, unless the blood be remarkably sizy. I cannot, however, refrain from taking notice of a more important point, as regards the stream of blood, namely, its remedial influence on disease. In venesection for the relief of inflammation, our object

is not simply to diminish the quantity of circulating blood, but to diminish the action of the heart; and it may be affirmed that twelve ounces of blood, drawn so quickly as to produce an immediate and decided effect on the pulse, will prove much more useful than a considerably larger quantity stolen away so slowly that the heart accommodates itself more easily to the loss. In a case of plethora, or apoplectic congestion, the same reasoning does not apply, because the absolute quantity of blood to be drawn is more to be considered."

Our limits will not permit us to extend this analysis of the interesting reflections of Dr. Scudamore upon the subject of venesection, to a greater length: we pass on, therefore, to give a short account of his observations upon the use of a *saturated solution of alum* as a styptic to bleeding vessels. An artery of considerable size in a dog was laid open, and bled vehemently. In comparative trials, our author used saturated solutions of acetate of lead, sulphat of copper, sulphat of zinc, and of pure nitrat of silver; tincture of galls, nitric acid diluted, and Ruspini's styptic, without checking the hæmorrhage. On pouring the alum solution on this vessel, he was "equally surprised and gratified to witness, that in about a quarter of a minute the bleeding entirely ceased; and that it was thus effectually restrained by means of the tenacious coagulum so promptly formed, operating as a plug to the mouth of the vessel, was proved several times by sponging away the coagulum, seeing the violent renewal of the hæmorrhage, repeating the application with perfect success, and so on."

In several cases of wounds, hæmorrhoids, &c. he has had opportunities of witnessing the same power of this remedy over hæmorrhage.

In cases of menorrhagia, especially of the passive kind, he apprehends that an injection of the same solution would be attended with the most important advantage; and under certain circumstances of hæmorrhage, after delivery and the removal of the placenta, he thinks "it will promptly prove a remedy for a state of imminent danger, when other means

would probably fail." He recommends the injection to be used tepid; while, at the same time, cold may be applied externally to the abdomen in the usual manner.

He does not claim any credit for the use of alum as a styptic, but merely for the introduction of it in the form of a saturated solution.

He has met with some opportunities of giving alum internally. In a case of hæmoptysis, and in another of hematemesis, it succeeded perfectly. In each instance the hæmorrhage ceased entirely after the administration of a few doses of the medicine.

We cannot conclude this review without acknowledging our obligations to the author for much new and useful information, which we have derived from a perusal of his pages. We consider this work as an important contribution to physiology; and feel confident that a republication of it in this country would be a benefit to the profession. M.

Art. V.—*Essays on Fevers and other Medical Subjects.* By Thomas Miner, M. D, and William Tully, M. D. Middletown, Connecticut. E. and H. Clark, 1823; 8vo. pp. 484.

It is impossible to read this book, without feeling some degree of disgust and mortification at the disrespect and contempt which the authors evince, in almost every page, for a great majority of their medical brethren. There runs throughout the whole work, a strain of invidious and ungracious remark, which we cannot but condemn as offensive, unbecoming, and injurious to the dignity and interests of the profession. Were we to receive our impressions from the authors of this book, we should be led to regard the generality of physicians as deplorably ignorant, rash, and incompetent. The epithets of ignorant, prejudiced, mad, barbarous, malevolent, and temporising, are dealt out with a prodigality which we should not expect from one who cultivates and practices a liberal profession. Notwithstanding all this, however, we

are quite ready to confess that there are many sound, practical, lessons taught in this book, and that on several important points of pathology and practice, we fully concur with the authors.

The first essay in this volume, is "*On the Resolution and Treatment of Fevers,*" by Dr. Miner. The author's object, in this essay, is to show that, contrary to the opinions of some eminent practitioners, fevers, when once formed, do not necessarily run on to their natural termination, but that they may frequently be arrested, or "cut short" in their course by a judicious treatment in their early stage. "By far the most prominent obstacle," says Dr. M., "to improvement in practice, is the opinion which has had too much influence ever since the days of Hippocrates, that every fever must run its course; that it can neither be broken up at its access, shortened in its duration, nor have its symptoms materially mitigated by the interference of art." In the preceding number of this journal, we have expressed our sentiments on this subject, in the review of Dr. Smith's essay on typhous fever; and we merely remark in this place, that there is no point of practical doctrine of the truth of which we are more fully persuaded, than of the practicability of occasionally arresting or shortening the duration of fevers. Upon this subject our sentiments are fully in accordance with those of Dr. M.; and we are no less ready to acknowledge our confidence in the plan of treatment which he recommends as the most effectual way for arresting the progress of fevers in their early stage. "*Slow and moderate purging with calomel,*" during the first stage of fevers, constitutes the essential feature of our author's practice. "This is the only one," he says, "of all the common devices for breaking up fevers at their access, which in my practice has been attended with any degree of certainty. The calomel mixed with any mild syrup, but not in the form of pill, should be administered in *small doses*,—from two to five grains every two or three hours, according

to the circumstances of the patient, till sufficient catharsis is produced. If possible, the calomel should be given in such doses, and at such intervals of time, as to be retained in the stomach and bowels twelve hours, before it produces the operative effect. But if catharsis does not take place in eighteen hours at farthest, it must be assisted by castor oil, or some other mild vegetable purgative. At the same time, it is proper to pursue a moderate diaphoretic regimen, such as pediluvium, tepid aromatic, and diaphoretic drinks, fomentations and sinapisms, or epispastics, as the local symptoms require."

In cases attended with diarrhœa, or where the calomel passes too rapidly through the bowels, the author advises a "single medium cathartic dose of calomel, with a sufficient quantity of opium to stay it, at least, for twelve or fifteen hours." Although it is only in the incipient stage of the disease that this plan of treatment is especially effectual in arresting its progress, yet it will sometimes succeed, says the author, in effecting a resolution of the fever even after it is completely formed. It is evident, as the author observes, that the peculiar advantages of this practice do not depend merely on the purgative effect of the calomel, but mainly, no doubt, on the general specific influence of the medicine on the system. We have ourselves often employed calomel in the *early* stage of typhoid and bilious remittent fevers, both with a view to its purgative and specific operation, and in many instances we have noticed that as soon as the constitutional impressions of the medicine became visible, the disease readily yielded. Useful as a moderate mercurial action, in connexion with gentle catharsis and other antiphlogistic means is, in the incipient period of fever, yet we have more than once observed the fact mentioned by the author, that calomel, given in the latter stages, so as to produce its specific impressions on the system, has a tendency to aggravate the disease, and to render convalescence tedious and troublesome. In the congestive form of fevers, calomel is a remedy of great value. Dr.

Armstrong observes, in relation to its efficacy in cases of this kind, that he had for a "long time overlooked one of the principal effects of calomel in congestive fever, and that it was at last only forced upon him by patients almost invariably recovering with rapidity when ptyalism was excited. The power which calomel has in equalizing the circulation, is no where more conspicuously displayed than in diseases of a congestive character. Before its exhibition the skin will be cold, wan, and shrunk, the pulse feeble or oppressed, and the whole system apparently relaxed, but as soon as the mouth is made sore from its influence, the skin becomes warm, reddish, and distended with the vigorous circulation, while the pulse is full, soft, and strong, and the general energy, in a great measure, restored. The liver is intimately concerned in the pathology of congestive fevers, and for the first day or two the alvine evacuations from calomel, will commonly be found either as dark as tar, or whitish and slimy; but they speedily become natural when ptyalism takes place, and a copious secretion of bile almost invariably augurs a favourable issue." (*On Typhus Fever, p.* 167.)

In endeavouring to bring about a resolution of an incipient fever, it is by no means necessary, or even proper, to produce *ptyalism;* all that is required for this purpose, is a slight general mercurial impression. "The cupreous taste, the slightest swelling of the gums, or sloughy whiteness, or erosion of the margin, and soreness of the teeth, the most moderate degree of the peculiar fleecy appearance of the tongue, and fetor of the breath, which are among the first visible effects of mercurials, will be amply sufficient." This is, however, not all that is necessary for the complete removal of a fever. As soon as the morbid action is changed, and the disease broken up by the influence of the calomel, recourse, says the author, should be immediately had to *tonic remedies,* and the calomel be discontinued. If, says he, the mercurial be continued through the course of the disease, as

is often done, it is apt to produce "an *artificial state*, in many instances as unmanageable as the original affection."

If, after all, we fail in "breaking up" the disease during the first few days, it then becomes necessary to bring the system gradually to a proper state for the administration of tonics, by "the remaining *preparatory* treatment.

"Whatever may be the opinion, says Dr. M., "with respect to *critical* days, it is certain that all our late typhoid fevers have very regularly observed *weekly* critical periods. It is an old and very just remark, that in all *regular* diseases of this description, they are generally but little benefitted by the *bark*, and articles of a similar tendency, till after a prominent, critical, or semi-critical change: that is not until morbid irritability of the stomach and bowels on the one hand, or torpor on the other, is overcome, local pains and other urgent symptoms relieved, and some time in every twenty-four hours at least, the tongue is moist, and the skin and other excretories are free. There are various means which are adopted for the purpose of producing this particular state of the system. This kind of crisis, however, by the usual management, does not generally take place until the end of the second week, when the patient is too frequently so much exhaused, that tonics are apt to produce very little effect, or the system is still so irritable that they cannot be borne at all. But it very certainly occurs, at the end of the first week, and occasionally some days earlier, especially in the more severe and rapid cases, when the following *alterative course* or plan of preparation, in addition to the efforts already made in the attempt at *breaking up* the disease, is properly executed."

This plan of preparatory treatment consists, according to the author, in the cautious administration of *small* doses of calomel, in combination with opium, and, where the skin is dry, ipecacuanha, during the first week. When by this treatment "a partial crisis, or semicritical change," is effected in the disease, "we must immediately enter on the supporting course, and except in a few obstinate cases of local inflammation, instantly suspend the calomel."

"This compound of calomel, opium, and (when necessary) ipecacuanha," says the author, "in order to produce its full effect, should be given at *short* and *regular* intervals, so that the system may he *constantly kept* under the combined effect of the whole of the articles. Each succeeding dose, therefore, must be administered a little time before the effect of the preceding has ceased.

"Before dismissing the subject of calomel, it is necessary to enforce one important point with respect to the administration of this article, viz:—That there is no certain dependence to be placed upon its use, either as a slow and moderate cathartic, or as an alterative, unless the patient will submit to an appropriate diaphoretic regimen. He must confine himself to bed; and, at the same time, use tepid, aromatic, and diaphoretic drinks, light liquid food, such as broth, milk porridge, arrow-root, or the lighter vegetable and animal, mucilaginous and gelatinous, preparations. Pediluvium, or warm bathing, sinapisms, and, in many instances, epispastics are indispensable auxiliaries. There can be no exceptions to this regimen, unless in the extreme hot cases of putrid fever, and in violent synocha."

The remainder of this essay is devoted to the consideration of the proper course to be pursued in the latter periods of fever; and of the value and mode of employing tonic remedies after the preparatory, or what, according to the author's method of cure, may be called the mercurial treatment, is over. Dr. M. is decidedly in favour of a tonic course of treatment after the morbid action has been changed by the use of calomel. In taking leave of this essay, we may remark, in a general way, that it contains a great deal of what we deem sound doctrine, and important practical observation. We are at the same time compelled by the duties of our function to declare, unreservedly, that the tenor of reproachful observation, and of violent and spiteful condemnation, which is kept up throughout its whole extent, operates powerfully to do away its respectability and usefulness.

The second essay in this volume is entitled: "*Medical authors are rarely practical physicians.*"—Dr. Miner seems

to have but a poor opinion of the majority of medical authors. He thinks that "the generality of the books which we read, and of the systems which are taught, are fifteen or twenty years *behind* the actual improved state of medical science:"—and the reason, as he alleges, is, that "most medical publications are either the speculations of the young, who lack employment, or the *recollections* of men who have, in a good degree, retired from business, or the theories of professors, who sit at their ease in the academical chair."

That many books are published by writers who are not practical physicians, is certainly true, but we are by no means willing to allow that the majority of publications, professedly practical, come from men not practically conversant with diseases. Works written by *young and inexperienced men*, and "theoretical professors," may in general be of little value as practical guides; but we see no justice in the disparagement which the author casts on "the *recollections*" of those who, after a long course of practice, retire from business, and publish the result of their abundant experience. Books that emanate from sources of this character, are, in fact, commonly the most valuable treasures of our literature. The observation of our author, that the information which books of this kind contain, is, for the most part, many years behind "the actual state of medical science," is not correct, as a general remark, and proceeds, as we believe, from an erroneous notion of the nature of medical experience, and the true advancement of our science. The progress of practical medicine is far from being so rapid, that the result of experience, published by a veteran physician on his retirement from practice, will be far behind "the actual improved state of medical science, in the short period of eighteen or twenty years." He who has perused, with proper attention, the old standard works of our profession, will, we apprehend, confess, that, although on many points of pathology and practice we have grown wiser than our forefathers, yet we are nevertheless often reminded, in their writings, of old truths, ne-

glected for the novelties of the day, and of modes of practice which it would be our interest to adopt and to cherish.

It is quite evident from this essay, that, although the author by no means lacks confidence in himself, he is, nevertheless, abundantly sceptical as to the merits of others. According to him, age is too timid, youth too modest, and "learned professors" too speculative, to expect from them a plain, open avowal of their sentiments, and an unvarnished statement of their experience. Added to this, he thinks that authors are too fond of popularity to *offend* by proposing new and unpopular opinions; or too much under the ban of *reviewers* to venture "to publish facts and observations in opposition to those which are generally received." Through the multitude of reasons which he gives us for distrusting the statements and opinions found in the majority of medical publications, we cannot but perceive how well the author is satisfied with his own intrepidity and sagaciousness,—a persuasion which, by the bye, we have no right to censure, except as it shows a self-sufficiency on the one hand, and a contempt on the other, which, to say the least of it, is by no means calculated to conciliate the respect and confidence of others.

In the third essay, the author treats of "*The diseases on the river Connecticut.*" "An *accurate* history," says the author, "of the epidemics which have appeared within the last fifteen years, on the Connecticut, from its mouth to the boundary of Massachusetts, would give more light on typhoid diseases, than all that has hitherto been published on this subject from the age of Hippocrates." Since the year 1807, fevers of a pure sthenic character have been exceedingly rare in Connecticut. From that period, when an influenza, "more universal than any within the memory of man, prevailed, and which was of the most decidedly asthenic character," all the acute diseases on the Connecticut, "have been asthenic, and progressively increasing in the severity and irregularity of their character."

"Our modern fevers," says, Dr. M., "assume every imaginable form and degree of violence and irregularity; from the influenza, which frequently admits of patients attending to their ordinary business,—to the spotted fever, which sometimes destroys life in six hours. They all, however, agree in *one* particular point, the *absence of the strong—hard pulse.* They all, likewise, agree in their *general* mode of treatment; an alterative, diaphoretic, exciting and supporting course; varied indeed, according to the symptoms, from the infusions of thoroughwort, hops, and chamomile, or wine-whey, on the one hand, to opium, alcohol, cantharides, arsenic, oil of turpentine, capsicum, and phosphorus on the other. The difference in the severity, violence, and rapidity, in different towns, and in different seasons, is extreme. In some seasons our fevers are so mild, that almost every one will recover under moderate treatment; in others the utmost decision and vigilance in the treatment are demanded, and after all there will be a number of unsuccessful cases. No one, however, can well realize their *typhoid* nature without *having seen them.* Nor can the speculations of strangers or occasional visitors, avail against uniform experience."

A corresponding change in the prevailing diathesis of febrile diseases, has, we believe, been noticed during the period referred to, throughout, perhaps, every section of this country. Nothing is more common than to hear our old practitioners say, that fevers do not now admit of the active depletory measures which were necessary during the latter period of the last and the few first years of the present century. *Wolfart*, a German writer of eminence, has attempted to account for the gradual conversion of the sthenic into the asthenic diathesis, by the changes wrought in the human constitution by the influence of climate, education, government, and above all, customs and modes of living, and the increasing and enfeebling power of luxury and sensual gratification in every shape. These causes, as they evidently tend to diminish the physical energies of the human body, must, as he believes, necessarily give to its diseases a correspond-

ing character of weakness. We are not prepared to adopt this doctrine in its full extent, but there can scarcely be a doubt, that something at least is contributed in this way to the phenomenon in question.

The fourth essay of this volume is entitled, "*Fallacy of popular reports on Medical subjects.*" In this essay Dr. Miner makes some additional remarks on his peculiar practice, lest, as he observes, he should be misunderstood, "or have his opinions wilfully misstated." After the preparatory process is accomplished by moderate purging with calomel, "the ordinary cases of regular nervous fever," he admits, require only "a light and moderate, though uniform support of wine, cinchona, and small doses of opium."

"But where the powers of life, from the *original* malignity or severity of the disease, or from previous neglect or mismanagement, either by the physician, patient, or nurses, are almost exhausted,—the last drop of oil nearly burnt out,—and the expiring wick only glimmering in the socket,—as long as there is sufficient excitability remaining for medicine to act upon, that physician is a murderer who stands by, and does not make the strongest exertions, with the most efficient articles. In such extreme cases I do not hesitate to advise cantharides, capsicum, alcohol, mineral solution, oil of turpentine, or even phosphorus, and, in a word, the combined action of several of the most highly exciting and tonic articles in the materia medica."

There can be no doubt of the correctness of this practice, but there is certainly nothing peculiar in it to Dr. M. We apprehend every man of common sense would stimulate,—and that too as vigorously as possible, in cases "almost exhausted,—where the last drop of oil is nearly burnt out,—and the expiring flame only glimmering in the socket." We can inform our author, that even in this city, notwithstanding the "*ignorance, rashness,* and *barbarity*" of some of its most eminent physicians, this practice is pursued as the only one which common sense and experience dictates in cases of this kind.

Medical Facts.—This is the title of the fifth essay. The author censures physicians, and perhaps justly, for not reporting their unfortunate cases. He asserts that he has lost but four cases of typhoid fever during the period of eight years. This is certainly a very small number, and must be regarded as strong evidence in favour of the excellence of the practice which he pursues, a practice to which we give a decided preference.—The author states that he has never seen a case of acute disease, in which mercury, whether employed internally or externally, has produced any "*permanent ill effects.*" He is a determined foe to the lancet, and the whole array of antiphlogistics. He condemns, positively, the opinion entertained by some, "that there is some *truth* and some *mistake* on either side,"—namely, the antiphlogistic and tonic plans of treating typhus. "The fact is," says he, "that no compromise is admissible." We are nevertheless fully persuaded that there *is* some "truth and some mistake on either side," in relation to these two modes of treating typhoid fevers. Ready as we are to accede to the paramount excellence of the alterative plan in the commencement and the supporting treatment in the latter stage of fevers of this character, we are not the less willing to acknowledge, that in the beginning of the disease, antiphlogistic remedies, properly so called, and even bleeding, may be sometimes employed with decided benefit. Our author vituperates those who show too much dread of the employment of tonics and mercury, and we think that he is equally liable to censure for his unqualified condemnation of the opposite class of remedies.

Chap. VI. *On Spotted Fever.*—The author's object, in the present chapter, is to show that evacuations of all kinds, except diaphoresis, by stimulating applications, are injurious in this rapid and deadly disease, and that no time is to be wasted "in useless preparation, but immediate recourse to be had to the most decided counteracting and supporting course from the very access."

Spotted fever, according to Dr. M. "belongs nosologically to the passive phlegmasiæ of the brain." No spontaneous reaction, he says, takes place in this disease.

"By an energetic, exciting treatment, at its access, this disease is frequently broken up within twenty-four hours. When we fail in producing a resolution, and only make such an impression as to moderate the violence of the symptoms, it usually continues seven, or in some instances fourteen days, requiring the same course of practice, according to the urgency of the symptoms, as is demanded in the supporting stage of typhus."

Excessive sweating is not to be encouraged, although a strict attention must be paid to the support of the proper temperature of the body by the application of external heat, frictions, and fomentations. *Morbid* sweating is sometimes found to attend this disease throughout its *whole course.*

Essay VII. *On Experience.*—This is a very confused chapter. It contains some good observations, mingled up with a great deal of invidious remark and dashing censure. The author speaks fluently of the ignorance, rashness, and stupidity of physicians. He thinks "that a prominent cause of the *deterioration* of medicine in the United States is the combination of the two professions,—physic and surgery." Surgeons, he says, are generally deficient "in the discrimination of diathesis, type, crisis, pulse, stage, and temperament;" they are almost "inevitably led to adopt and rely upon a mere succession of individual *dashing* and desultory measures." In Europe the practice of physic, according to Dr. Miner, seems to be in a state of utter confusion. "Judging from the writings of Hamilton, Hey, Clutterbuck, Johnson, Welsh, and Armstrong, and most of the periodical works, modern physicians appear to be guided by no definite rules of practice in acute *diseases.*" "They appear to have lost all accurate knowledge of *type*, temperament, and *diathesis;* of the distinction of *active* and *passive* inflammation; of the

difference between *inflammation* and *irritation* between the *quantity* and the *quality* of diseased action; or between irritability and torpor." They waste the vital fluid "by *gallons*," are ignorant of the true powers of some of the most useful medicines, and, in a word, of every thing which Dr. Miner, and his partizans, appear to know so well.

We cannot help remarking, by the way, that while our author is thus earnest in pointing out the deficiency of modern practitioners, and in exposing their unbounded rashness, he is far from showing any diffidence of his own judgment and practical abilities. In *his* hands nothing is more manageable than *incipient* consumption. He is astonished at the large proportion of deaths from this disease in the populous cities of our country, and he ascribes it to the "reducing plan of treatment" which, he asserts, is so generally adopted by our practitioners. With calomel, opium, sanguinaria, and a suitable course of mineral tonics, he finds no difficulty of curing the disease in its incipient stage. With these remedies he asserts "that he has never failed of curing the complaint in more than *one* instance." This success is certainly very extraordinary; so extraordinary indeed, that we must ask the author's pardon for being inclined to doubt the accuracy of his diagnosis.

The remaining essays of this volume, by Dr. Miner, are devoted to the consideration of the pulse; stage of fevers; type of fevers; crisis and critical periods; diathesis; counteracting and coinciting agents. We have not room to enter into a particular analysis of these chapters, all of which, however, are interesting, and replete with judicious observations. In relation to the small, quick, and hard pulse, the author observes:

"The frequent, quick, small, and hard, or wiry pulse, though too frequently considered as indicating a phlogistic state, denotes nothing more than the addition of *irritation* to exhaustion. This pulse is often found in sub-acute pneumonia, phthisis, sub-acute

rheumatism, idiopathic, hectic, putrid, and yellow fevers, when they have been injudiciously managed."

In speaking of the combination of quickness, smallness, and hardness, in the pulse, as connected with active gastric and enteritic inflammation, the author remarks:

"It is to be feared that the almost universal prevalence of the idea, that these *inflammations* are *necessarily* attended with this pulse, has prevented all investigation of diathesis in these diseases, and consequently led to discriminate practice. Suffice it in this place to say, that genuine asthenic inflammations of the stomach and intestines do *actually* exist, which do not require an antiphlogistic treatment, and which recover with much greater certainty under a different management."

In the chapter on the "stage of fever," Dr. Miner makes the following observations in relation to the employment of calomel in irregular or malignant epidemics:

"In irregular or malignant epidemics, much decision and caution are required in the administration of calomel, or it is of no avail. Where there is an early moderate determination to the brain, the patients reluctantly submit to the proper regimen. In this variety calomel, in full doses, must be combined with opium, sufficient to prevent its having any more than a laxative operation. The mouth is to be touched as early as possible, and the mercurial action is to be resolutely continued till a manifest sub-critical change supervenes. Rapid cases require a still more decided course of calomel and opium, or the calomel is worse than useless. The *instant* a sub-ptyalism is perceived, tonics and excitants must be efficiently prescribed.

We must refer the reader to the book itself for many excellent observations on "the type of fevers," on "crises and critical periods," and on "diathesis." That Dr. Miner is an able physician, and an accurate observer of diseases, is abundantly evident in these chapters. Notwithstanding the censures which we have been reluctantly obliged to pass on the

intemperate observations of our author, we do not hesitate to say, that, in our opinion, this book contains much sound practical information. That there are points of pathology and treatment recommended in it, which might be fairly, and we think successfully contested, must indeed be affirmed; but in the leading doctrines, both practical and pathological, advanced by Dr. M., we freely confess our entire confidence.

Want of room obliges us to postpone our remarks on Dr. Tully's part of this work. We will merely observe that Dr. Tully has indulged less in those obnoxious observations on the opinions and practices of others, which are so offensive in the essays of his colleague. In our next number we shall devote some pages to Dr. Tully's portion of this volume. E.

ART. VII.—DR. E. L. JOURDAIN *on the Yellow Fever of Port du Passage.*

IN the number of *Annales de la Medecine Phisiologique*, edited by Dr. Broussais, for December 1823, Dr. E. L. Jourdain, chief physician of the military hospital of Daxhas, published a very interesting paper on the yellow fever which prevailed at Port du Passage during the months of August and September, 1823. This port is situated at seven leagues east of Bayonne, one league west of St. Sebastian, and at two leagues south-west of Ernani. It is one of the finest in Europe, though at present considerably neglected. It is well fortified by nature and art, and covered towards the land by high mountains and rocks. The bay, which is large, and on which are situated several villages, communicates with the sea only by a small passage situated between two rocks, and affording passage but to one vessel.

The Passage is a small town, divided into two parts by the canal which forms the entrance of the bay. The Eastern portion of the town, called at present St. John, is that in which the yellow fever reigned. It consists of one street

only, which occupies the whole extent between the bay and the eastern mountains. This street runs from north to east, —and after forming an angle runs from east to south. It is very narrow in some parts, not exceeding four or five feet in width.—The houses, which do not amount to more than one hundred and twenty-five, are small, obscure, and ill ventillated. Those placed against the mountain are more especially so, and for the most part have no privies. The houses situated near the canal are larger and cleaner.

The population of St. John is, in general, eight hundred; though at the time of the appearance of the yellow fever, it amounted to three thousand, owing to the emigration there from St. Sebastian.

Notwithstanding some insalubrity, Dr. Jourdain says that St. John is represented to be the healthiest spot of that part of country; acute diseases being rarely met with, and consisting of catarrhal affections in winter, and simple bilious fevers during the summer months.

The disease appears to have been introduced into this place by a vessel from Havana. The detail of this introduction we shall give in the language of Dr. Jourdain. "The *Donostierra* left Havana the beginning of June, with twenty-one men on board, including the captain and five passengers; —the cargo consisting of sugar, coffee, tobacco, yellow wax, a barrel of honey, and a small box of preserves. Ten or twelve days after the departure of the vessel, one of the sailors died; his death was attributed to a fruit called in the country pina, of which he had eat a large quantity, and to the brandy of sugar-cane, which he drank to great excess. A few moments after his death, he discharged by the mouth a large quantity of froth.

After a passage of thirty-five days, the vessel touched at Corona. The captain reported this event to the medical junta, in consequence of which the vessel was ordered to quarantine ten days. At the expiration of this time, the vessel proceeded to St. Ander;—remained six days in that port, and

next directed its course to the Port of the Passage, where it arrived on the second of August, at 7 o'clock in the morning. It was anchored near the little place called De la Piedad, situated in the centre of the town. The crew was in good health.

The vessel was unloaded on the sixth of August, and on the nineteenth carpenters were employed to rebottom it. The first individual attacked with the disease was Manuel Ali, the custom-house officer, who had slept several nights on board.—He was taken on the fifteenth, and died on the seventeenth, with all characteristic marks of the yellow fever.

The carpenters were soon all affected with the same disease, of which the greater number died. On the twenty-third and twenty-fourth of August, two women, who had communicated with the vessel during the week, and who occupied the house nearest to it, took sick and died, one on the twenty-fifth, the second on the twenty-sixth.

From this moment the disease spread to all the houses in the vicinity of the vessel, this latter being evidently the source of the infection. Dr. Jourdain does not think the infection could be referred to the cargo, but to the interior of the vessel itself; because it was only when its right side was opened that the disease appeared. Ali, it is true, died before this operation was performed, but from the nature of his office, he was obliged to visit every part of the vessel, and was therefore exposed to inhale all deleterious vapours found in the interior of the vessel, even before the part which contained them was opened. The vessel appears to have been rotten. It had been on the coast of Africa three or four years before, and had had negroes on board.

There does not appear to have been any case of contagion during the prevalence of this disease, every individual having taken it in the place of infection, either on board of the vessel, or in the houses situated in its immediate vicinity. Dr. Jourdain, however, is of opinion, that a surgeon and two grave-diggers took it from handling the bodies of those who

had died of it;—but as they had, in all probability, visited the infected district, it is difficult to decide satisfactorily this question. No individual affected with this fever, and transported to the neighbouring villages, or in the other parts of the town, communicated it to those around them.

This account is interesting in as much as it proves that a vessel in a filthy condition may, when placed under circumstances favourable to the development of the yellow fever, become a source of infection; a fact which many of our antagonists have almost invariably neglected to notice, when endeavouring to demonstrate the importation of this disease.

But this paper is also important in a practical point of view. Dr. Jourdain gives us a table of the patients,—containing the principal symptoms, treatment, issue, and post mortem examination. It results from this, that there were eighty-five cases, of which sixty-one were cured, and twenty-four died, not including several who took the disease in the infected spot, and died in the neighbouring villages.

Dr. Jourdain's observation tends to prove some of the propositions of the physiological school of France, of which he appears to be a disciple. He remarked: 1st,—That the greater number of patients, although violently affected, and presenting the most alarming symptoms, retained to the last their muscular strength. 2d,—The yellow colour of the skin invariably increased after death. 3d,—General bleedings were fatal in the greater number of cases. 4th,—Emetics invariably aggravated the disease, especially when administered in the beginning. 5th,—Irritating glysters of salt and vinegar were hurtful. 6th,—Soothing, mucilaginous and emolient substances, were beneficial as drink, glysters, cataplasms, or fomentations. 7th,—Oleaginous substances were advantageous. 8th,—Leeches, when applied in great numbers, in the beginning of the attack, or the epigantric region, invariably arrested the progress of the disease, but very often they were only applied in small numbers, or else after the use of dangerous means, such as general bleedings, or emetics. 9th,—

External stimuli, such as sinapisms and blisters, were very useful in the second period of the disease, as powerful resultives. 10th,—Internal stimuli and barks were not employed by any of the physicians who treated this disease in this plan, being all convinced that it was a disease more or less inflammatory.

Art. VIII.—*On Dislocations of the Vertebræ of the Neck,* By Ph. V. Walther.*

Case 1.—In July, 1819, a child, aged three years and a-half, was brought to the Polyclinic Institute, with dislocation of the two lower oblique processes of the third or fourth cervical vertebra. Thirty-six hours before it was seen by Dr. W. the child was taken by the head and raised perpendicularly from the floor, in which position it was held about a minute and a-half, struggling and screaming violently. When it was put to the floor again, its head stood backwards so as almost to touch the part between the shoulder plates. In this position the head remained immovably fixed. The child, when brought to the institution, complained of pain in the head and neck. There was no disturbance of the respiratory function; nor did the little patient experience any material difficulty in swallowing. Dr. Walther was surprised to find that although manifestly a case of dislocation of one of the cervical vertebra, there was not the least difficulty of breathing or paralysis present. Dr. W. immediately proceeded to reduce the dislocation. The child was held up in a horizontal position by three strong assistants. One of them made counter-extension by a firm hold of the hips; the second held the shoulders back and supported the body of the child; the third laid hold of the head and made extension, at first in the direction of the dislocation, and afterward in the natural direction of the neck. In doing this, he at first, while applying

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* Journal fuir Chirurgie und Augenheilkunde. Von Dr. E. Graefe un Dr. P. J. V. Walther.

the extension, pressed the head slightly backwards, and then gradually increasing the extension, brought it by degrees to its natural position. In this manner the dislocation was reduced in the first attempt; the head again stood erect, and the patient could immediately move it freely in every direction.

Case 2.—A carpenter fell from a high scaffold. He struck the hard ground with the back of the head. He remained dead on the spot; not the slightest motion of his limbs having been observed by those who were present. Twenty-four hours afterwards his body was dissected. The medulla oblongata was found torn off about two lines below its exit from the cavity of the cranium. The retracted ends of the divided cord, were separated several lines. There was no effusion of blood into the cavity of the spine.

Case 3.--Gilsdorff, aged twenty-one, while driving an empty cart, fell, and one of the wheels went directly over the back of his neck. He was taken up and carried into a house; the surgeon who was first called in, pronounced the injury slight, and of no serious consequence, regarding his inability to move his extremities as the effect of excessive fright. Another surgeon, however, who afterwards saw him, formed a more correct opinion of the nature of the injury, and had him sent to the surgical institution on the third day after the accident. *Symptoms.*—Paralysis of the upper and lower extremities; in the lower the paralysis was perfect with anaesthesis; in the upper the sensibility remained as usual. There was, also, paralysis of the abdominal muscles, with weak and stertorous respiration, retension of urine, involuntary stools, and a strong and irregular pulse. Neither stupor, delirium, vomiting, nor hiccough was present. He was entirely free from pain, nor were his sight and hearing in the least affected. He was freely bled, leeched over the tract of the spine, &c. &c.; but all to no advantage. He died on the night after he was brought into the Institute.

On dissection the fifth and sixth cervical vertebra were found to be separated from each other several lines, and the

capsular ligaments, as well as the ligaments between the arches and spinous processes of these vertebræ were torn off. There was a coagulum of extravasated blood between the dura mater and the bony cavity throughout its whole extent. The spinal marrow immediately under the third and fourth cervical vertebræ was lacerated. In this situation, also, the pia mater, on the left side, was torn, and a small portion of the medullary substance protruded. A coagulum of blood was here found between the pia mater and the spinal marrow.

There can be no doubt, observes Dr. W., that in the first of these cases there was dislocation of one or more of the cervical vertebræ. In relation to the cause by which it was produced, this case coincides entirely with the one related by *Petit*, where a child raised perpendicularly and held for a short time suspended by its head, immediately expired.

Boyer (Traité des Maladies Chirurgicales, etc. Tom. 4,) thinks that in Petit's case there was luxation of the first and second vertebræ of the neck; and that the *processus dentatus* having slipped out from under the transverse ligament occasioned immediate death, by compressing the medulla oblongata. As no dissection was made of this case, it may still be doubted, observes Dr. Walther, whether this was the case or not. Dr. W. infers that in his case, a luxation of the processus dentatus could not have existed, since such an occurrence must have necessarily proved immediately fatal. From the similarity of the causes in his case and in Petit's he thinks it will admit of a doubt, whether, even in this latter instance, such a luxation existed; and the second case related above, would appear still further to justify this doubt, since, although instantaneously fatal, the dentatus was not dislocated. Boyer, after having described the pathognomic marks of a dislocation of *one* of the lower oblique processes, asks "whether there is an example on record of the *two* lower oblique processes of a vertebra having been dislocated at the same time?" He leaves this question unanswered, and adds, that it is much easier to imagine such a dislocation, than to establish its existence by observation; its symptoms, effects and degree of dan-

ger are therefore as yet unknown. Dr. W. is persuaded that in the first of the above related cases, such a dislocation must have existed. The symptoms of this accident, as well as the degree of danger connected with it, may, therefore, he thinks, be deduced from this example. For as in a luxation of but *one* of the oblique processes of a cervical vertebræ the head is drawn towards the side in which the luxation exists, so, when the processes of both sides are luxated, it must necessarily be drawn directly backwards, giving a convex prominence to the forepart of the neck. He is the more convinced that his case was of this kind, since it does not appear that either a permanent distortion of the head *backwards*, or a dislocation of the two lower oblique processes of the same vertebræ had ever before been described by any one. It is obvious that the general symptoms and degree of fatality of such accidents must depend directly on the degree of injury which is inflicted on the spinal marrow. If it is not lacerated, and but slightly compressed, a reduction of the dislocation, will perhaps generally obviate dangerous consequences. Where the injury done to the spinal cord is great, speedy death is a necessary consequence.

Dr. Walther, therefore, divides dislocations of the vertebræ into *simple* and *complicated; simple* when the spinal marrow remains uninjured; and *complicated* when it is violently compressed, torn or lacerated.

The second case related by Dr. W. shows that the spinal marrow may be much injured—nay, even torn off, by external mechanical force, without either fracture or dislocation of the vertebræ. It is remarkable, he observes, that in some cases of mechanical violence to the head, the injury done to the brain or spinal cord is altogether confined to a particular spot, without any lesion of the surrounding parts. Thus he has known a fall upon the head to be immediately followed by permanent deafness; and in another accident of this kind a sudden and total loss of the sense of smell took place, without there having been the least injury done to the other senses, or to the understanding, in either case.

Dr. Walther combats with earnestness, and according to our views with much justice, the aphorism of Boyer, that the reduction of a luxated oblique process should never be attempted. Boyer apprehended great danger from such an attempt, thinking it much better to bear with the distortion which such a luxation produces, than to run the great hazard of fatal injury from an attempt to reduce it. Dr. W. observes that it does not appear that Boyer himself ever witnessed any injurious consequences from an attempt to reduce such a dislocation; for he does not fortify his opinion on this point by any observations of his own, but refers to the authority of his preceptor, Desault. But it would seem that even Desault himself had no experience on this head; for he does not in any of his writings mention the occurrence of such a case in his practice. "Who then," says Dr. W. "has observed the fatal consequence of an attempt to reduce such a dislocation?" The only case mentioned by Boyer, is reported by Petit Royal in the *Encyclopedie Méthodique.* This writer states that he saw a child, whose neck, after a fall, was turned to one side; a dislocation was supposed to exist, which was afterwards found to be the case; during an attempt to reduce it the child expired. There is, on the contrary, no inconsiderable number of cases on record, where the reduction of this species of luxation was effected without any dangerous consequences. Dr. W. quotes the following cases of successful reduction. Ehrlich, in his Chirurgical Observations, printed at Leibzig, 1815, mentions a case of this kind. Other cases are mentioned, Rust, (*Salzburg, Med. and Surg. Journ.* 1813, *vol.* 3.) Dorr, (*Medical Repository, Hexade II. vol.* 1.) Harrup, (*New London, Med. Journal, vol.* 1.) Ranby, (*Chirurgical Observations.*) Ruidiger and Sellin, in Smucker's Miscellaneous Works, vol. 1.

That this operation is unattended with danger, is by no means to be imagined; but it is equally evident, as Dr. W. observes, that Boyer's apprehensions were much too great; and that we should not in all instances, be deterred by his advise, from undertaking the reduction of luxations of this kind. E.

ORIGINAL DEPARTMENT.

ART. IX.—*On the Efficacy of Nauseating Articles, combined with Mercury, in promoting a salivation.* By JOSEPH KLAPP, M. D.

THE majority of physicians have, in the course of their practice, met with cases in which they could not obtain from any of the preparations of mercury, a salivant effect. Such failures, it is almost unnecessary to remark, are sources, not merely of disappointment, but of much mortification and regret, as the medical attendant will often have reason to believe, that if he could have induced a salivation, he would have succeeded in effecting a cure.

There are, no doubt, many causes which occasionally are brought into operation to prevent the system from being subjected to the specific influence of mercury, some of which have been accurately pointed out, and appropriate means suggested to overcome the difficulties. With such obstacles as a too excited pulse, a habit of body too plethoric, or a circulation feeble and asthenic, and a dry skin with or without fever, the practitioner is quite familiar, as well as with the methods to be resorted to respectively in such cases. Notwithstanding, however, the possession of this very useful and important experience, cases will now and then occur, in which the physician will still have cause to lament the insufficiency of his means to mercurialize the system. Without assuming to myself any unusual extent of experience, I can say that I have often met with cases, both in public and in private practice, where the insusceptibility to the salivant process did not arise out of any of the above mentioned circumstances; and in which neither bleeding, purging, opium, nor the warm bath, were of the least avail. Similar difficulties have no doubt been met with by other practitioners, who have felt as well as myself, a strong desire to know something of the nature of the cause which formed the obstacle. To this desire the physician is very naturally led by an encouraging supposition, that a knowledge

of the circumstance which renders his prescription inoperative, would readily lead him to a rational method of correcting or removing the impediment. With me, for a length of time, various plans were devised, and I must say, instituted with the most discouraging results. The natural intricacy of disease, the difficulty of exploring the real state of the different organs, and more especially our want of information as to the way in which the characteristic or specific influence of mercury is induced, jointly served to impose on the subject great difficulty and uncertainty. While thus situated, a few accidental occurrences supplied me with a clue to a train of further observation and inquiry; the result of which I hope presently to be able to show, has led to an extension of the dominion of the physician over a certain class of cases, which, by the use of other means, would have remained insusceptible to the mercurial action.

In several instances where an ordinary dose of calomel and jalap had been prescribed, the medicine, instead of operating as a cathartic, proved emetic, and it was remarked that the patients were affected with a salivation in a short time afterwards. I have, in the course of an evening, prescribed the above medicine in a case of bilious colic, and on visiting the patient the next morning, I have been told that the powder had caused severe vomiting, but had produced only an inconsiderable effect on the bowels, and the patient has been found in a state of perfect ptyalism.

In another case my attention was also excited in consequence of a very alarming salivation having been produced, in a short time after taking a single dose of ipecacuanha and calomel, with the view to an emetic and a cathartic effect.

These sudden and unexpected salivations surprised me, and led me to investigate the principle on which they had occurred. As the phenomenon had always been connected with, or preceded by vomiting, I did not hesitate to suppose, that this derangement of the stomach, had, in some way, been the cause. This reasoning brought to my recollection a fact

which had been observed not only by myself, but by several other practitioners of this city, that a ptyalism may, in general, be more promptly excited by the use of calomel, nitre, and tartarized antimony, a formula which is familiarly styled here the antimonial powder, than by the employment of calomel separately. Having proceeded thus far, it became necessary in the next place to inquire what connexion could subsist between a state of nausea, and the excitement of a speedy salivation? I am aware that this question may be differently answered by persons professing different theories of the modus operandi of mercury. Without wishing to enter into the discussion of the general question, I shall state that the facility which was impressed on the action of the calomel in the occurrences above referred to, did not appear to be owing either to a lowering of the pulse, or to any determination of the fluids to the external surface; because neither the pulse nor the skin had been in an unfavourable state for the excitement of the salivant process; further, the action of the calomel on the mouth occurred, I think, in too short a period of time for the agency of any causes operating so indirectly. The effect must have been brought about by some circumstance both powerful and direct in its influence; this I believe to have been a sympathetic connexion which exists between the stomach and salivary glands. To show that the sensation of nausea, even though variously excited, usually produces an increased action of the salivary organs, I have only to refer to the profuse discharge of saliva which takes place just before an emetic begins to operate, also to the symptoms of cardialgia, pregnancy, and a variety of other diseases. Even disgusting sights and odours, it is well known, will, at almost any time, set one a spitting. A majority of mercurialized patients whom I have questioned, have assured me, that a short time before they began to spit, or were sensible of soreness in the gums, they had been more or less sick at the stomach. Hence, nausea may be regarded as indicating the speedy action of mercury on the mouth, and the same

thing has been observed in the different ways in which a salivation is induced, whether by rubbing the preparation on the external surface, or by receiving it into the stomach.

It was thus by repeatedly observing a deranged state of the stomach to precede the action of mercury on the mouth and throat, that the idea was first suggested to me, in cases difficult to salivate, of exhibiting nauseating articles to promote a ptyalism. The result of this mode of reasoning, applied to practice, will be seen in the history of the cases presently to be submitted. A greater number of cases might have been detailed, but I have preferred confining myself to a few, though I trust the number will be sufficient to illustrate the practice. I am at the same time in hopes that they will prove the more acceptable, as they have either been drawn up or witnessed by respectable medical men, void of every prepossession from theoretical views.

Previous to the submission of the practice, it will be proper for me to state, that the sentiments set forth in this paper have been entertained by me during a period of several years, and that so long ago as 1819, the practice was fairly tried in the Philadelphia alms-house infirmary, under the inspection of a pretty large class of medical students, then attending to the clinical practice of the house. Dr. Paris, a late very respectable writer on pharmacology, notices the effect which the excitement of nausea has in increasing the susceptibility of the system to the action of mercury. It is his opinion that the absorption of mercury into the circulation is greatly increased when the stomach is in a deranged state. But I think what he imputes to an augmented absorption, may with more propriety be referred to the sympathetic connexion between the stomach and the salivary glands. Dr. Paris's book, containing his sentiments on this subject, was published at least three years after my practice had been devised, and publicly put to the test of trial. In advocating the opinion that mercury acts sympathetically on the salivary glands, I would not wish to be understood to deny the absorption of it into the cir-

culation, and the possibility of its acting on the heart, arteries, and secretory system, through the medium of the blood, as contended for by the above eminent writer, and many other modern physicians. That mercury, however, also acts independent of the circulation, through the medium of the nerves, and has its effects extended into other and distant parts, by the excitement of the sympathies, cannot be denied. Indeed I see nothing improbable in admitting that both the absorption and the sympathetic effects of mercury may be increased by a deranged or nauseated state of the stomach. Neither the consistency nor the value of the practical part of this communication, and it is this which I only feel anxious about, can be effected by the adoption of either hypothesis, or both. Under the first view we would say, that nausea, by reducing the pulse, would increase the amount of absorption, it being an acknowledged law "that the energy of absorption is generally in an inverse ratio to that of the circulation;" and the advocates of the other theory will recognise in a well regulated nausea, a very powerful instrument of extending, from the nerves of the stomach, the specific influence of mercury over the secreting vessels of the salivary glands. The constitutional operation of mercury may be owing to its absorption into the blood, but the facts I have before stated, sufficiently expose the existence of a sympathetic connexion between the stomach, and the mouth, and throat, through the excitement of which the sialagogue effect is produced. Besides these facts, an additional one occurs to me, which, as it is of an interesting character, I shall pay some attention to. I allude to the difficulty of salivating children. It is confessed that our acquaintance with the laws of the nervous system is too limited, and the doctrine of sympathy involved in too much intricacy, to enable one to advance any thing on this subject with much confidence; but as sympathetic or associated motions require no small degree of repetition, and some time before they can become established, it will not be regarded unreasonable to suppose, that

this association or consent of parts between the stomach and the salivary glands, has not had time to take place in infancy, or exists in too limited an extent to admit of a salivation being produced.

I shall now proceed to relate some examples of the mode of practice which has been inculcated. The four first cases were drawn up at my request, by a very ingenious gentleman, who was house-surgeon of the infirmary in which they occurred. They are given in his own language.

Case 1.—"Ann Moa, aged forty years, was admitted into the Philadelphia alms-house on the sixteenth of September, 1819, labouring under rheumatism, which affected principally the lower extremities and right arm, and was attended with some excitement of pulse, and heat of skin. The treatment during the four first days after admission consisted in the administration of sudorific medicines, and the application of blisters to the parts affected. On the twentieth of September, Dr. Klapp directed two grains of calomel to be given thrice a-day, with a view of exciting ptyalism. This treatment was faithfully persevered in till the first of October, without producing any effect on the mouth, when Dr. Klapp substituted a formula, consisting of eight grains of ipecacuanha, and two grains of calomel thrice a-day.

This, as was anticipated, produced great nausea, and on the third day after commencing its use, the patient's mouth was slightly affected, but though it was continued several days, profuse salivation could not be induced.

Case 2.—Patrick Burke, aged forty-eight years, had been cured of hydrothorax and anasarca of the lower limbs, but Dr. Klapp suspecting some remaining hepatic disease, directed, on the thirtieth of September, eight grains of ipecacuanha and two of calomel, three times a-day. This combination producing great nausea and frequent efforts to vomiting, the quantity was diminished one-half after the administration of the second powder, and on the third morning after the first exhibition, his mouth was slightly affected, and on the fourth salivation was free.

Case 3.—On the thirtieth of September, 1819, Dr. Klapp prescribed a formula, consisting of ipecacuanha two grains, and calomel half a grain, thrice a-day, to excite ptyalism in John Russel, a patient who had been labouring under rheumatism. This had the effect of producing a moderate flow of saliva by the fourth day, *i. e.* after the exhibition of eight powders.

Case 4.—On the twenty-seventh of September, 1819, Dr. Klapp directed two grains of calomel thrice a-day to John Hoover, a patient labouring under phthisis pulmonalis. This having been continued with scarcely any perceptible effect on the mouth, on the eleventh of October Dr. Klapp substituted four grains of ipecacuanha with one of calomel, which had the desired effect, the patient being found on the fourth morning salivated."

Case 5.—Miss L., aged seventeen years, affected with paralytic symptoms and amenorrhœa, was directed the use of calomel, with the view to salivate, in February, 1822. Twelve months previously, Dr. Gebbard, her physician, assured me he had made an unsuccessful attempt to salivate her. She then took calomel for two weeks without producing even the slightest effect on the mouth. Considerable debility supervening, the undertaking was abandoned. I, however, stated to the doctor, that this information did not discourage me, for although her disease remained the same, and her present debility considerable, yet on account of a symptom happening to exist at this time, which did not when the doctor had failed with the calomel, I flattered myself that our endeavours would now prove more successful. The symptom alluded to was great irritability of stomach, causing her frequently to vomit up almost every thing taken. From this view we were induced to renew the attempt. In two or three days the action of the mercury on the gums was evident, and in a week more the flow of saliva was very profuse. The mouth continued to be affected for three weeks and better.

Case 6.—S. S., aged thirty-eight, labouring under inflam-

mation of the brain, took calomel freely from the third or fourth of March 1822, but on the eighteenth, not having been able to produce more than a slight soreness of the gums, he was directed a grain of calomel with an eighth of a grain of tartar emetic every two hours. The first dose vomited him, and caused so much irritability of the stomach, that for several hours nothing could be kept on it. The next day he, for the first time, complained to Dr. Hewson and myself of his mouth being sore. At our next visit, which was on the morning of the twentieth, we were fully sensible of the effect the nausea and vomiting had caused in adding to the action of the mercury on the mouth. The same formula was continued. We visited him again in the evening, when the swelling of the face and inflammation of the mouth had increased so much, as to make it unsafe to continue the calomel any longer. He was ordered to be purged with calcined magnesia. After having taken, at different times, half an ounce, on the twenty-first he had four or five bilious stools. The swelling of the face and soreness of mouth had increased. The alvine discharge being unfavourable, the magnesia was continued. On the twenty-second, the mercurial action was so fully developed, that it was conceived unnecessary to resume the calomel.

Dr. Eberle has done me the honour to make particular mention of my sentiments on this subject, in the second volume of his valuable work on Materia Medica. He also records a case, showing the success of the practice, which, with some of his remarks, I shall take the liberty of quoting, as affording me considerable disinterested support.* "Of the efficacy of nauseating doses of antimony in overcoming the resistance of the system to the action of mercury, I have lately had a remarkable example. After having tried ineffectually for nearly two months, to bring a venereal patient under the spe-

* Eberle's Mat. Medica, Vol. II., p. 408—9.

cific influence of mercury, employed both internally and externally, I at length, by the suggestion of Dr. Klapp, gave him nauseating doses of tartar emetic, and by these means succeeded, in less than three days, in establishing a gentle salivation.

"Dr. Paris* suggests that nausea and other causes which derange the digestive organs, favour the salivant influence of mercury, by the power which they may possess, of "overcoming the resistance which nature, in a state health, makes to the admission of poisonous substances into the circulation." This explanation is, however, wholly hyphotetical; since it is perfectly ascertained that a state of health is generally the most favourable condition of the system for the specific influence of mercury. It is moreover predicated upon the supposition that remediate substances are more readily absorbed in a state of debility and disease, than in the vigour of health,—a supposition by no means countenanced by experience. Dr. Klapp, in a conversation which I had with him on this subject, suggested that the effects of nausea in favouring the operation of mercury, may be referred to the obvious tendency which sickness of the stomach has to determine the circulation to the salivary glands, and to excite their secretory action, as is shown to be the case by the increased flow of saliva which commonly takes place during nausea. There is, undoubtedly, a very intimate connexion between the stomach and the glands of the mouth; and it is not improbable that through this relationship between these organs, nausea may produce the effects which are here ascribed to it. This practice of combining nauseating doses of emetic substances with mercury, for the purpose of expediting its salivant operation, was employed by Dr. Klapp several years before Dr. Paris's observations on this subject were pubished."

Philadelphia, July, 1824.

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* Paris's Pharmacologia, p. 115. Lond. 3d edit.

ART. X.—*An Account of an Extraordinary Predisposition to Hæmorrhage in a Family of Children.* Communicated by GIDEON HUMPHREY, M. D.

IN the summer of 1817, while I practised medicine in Delaware county, Mr. and Mrs. G—— called on me with their child, aged fifteen months, labouring under hæmorrhage from laceration of the frœnum of the upper lip. The accident had occurred three days previously, and the child was very much exhausted from the loss of blood. They informed me that three physicians had been called, but that the means used had not succeed in checking it. The parts in the neighbourhood of the injury were considerably inflamed and tumefied. Resting satisfied that the common means resorted to in such cases had been tried, I determined to apply the actual cautery, the inflammation, &c., notwithstanding. This I resolved on, because, had I waited to reduce the inflammation, the child must have inevitably sunk under the discharge. —I therefore cauterized it sufficiently to form an eschar, which checked the bleeding, and the child afterwards recovered. The parents then informed me that they had lost two children, one aged eighteen months, the other about three years, by "bleeding" from very slight injuries. The first was by an accident similar to the one I had just treated, viz: a laceration of the frœnum of the upper lip, occasioned by a fall on the floor. That the same accident, and so uncommon an one too, should occur twice in the same family, may appear somewhat singular, but such was the information I received. The other case was an injury received on the forehead, not more serious, to appearance, than a scratch of a pin.—In each of these cases two "doctors" had been called, but all their efforts to check the bleeding had proved ineffectual, and both children died. When compression was used, the blood effused itself into the cellular tissue; the parts swelled, inflamed, &c., and as soon as the compresses were removed, the blood again oozed from the wound.—They informed me, that

the cautery had not been used in either case.—The case, then, to which my attention was called, was the third that had occurred in the same family.

In May 1818, I was called to R. W., brother to the above lady, a lad aged eleven years. He was bleeding from a cut with a penknife on the end of the fore-finger. It was small, but no means that the family could invent, would stop the hæmorrhage, which, though small, became alarming from its continuance I was called on the fifth day after the accident occurred, and applied a compress sufficiently tight to stop the discharge, but, on the second day after, I was again called to him. I found the hand very much swoln, inflamed, and very painful; the fore-arm much swoln, and the injured finger and palm of the hand were black from an effusion of blood;—upon removing the dressings, the blood again began to ooze from the cut. I now ordered means for reducing the inflammation, with simple dressings of lint to the cut; on the second day from this, the inflammation, &c., being in a great measure reduced, and the cut still bleeding, I applied the cautery which succeeded in suppressing it. In autumn of of the same year, the same lad had the second molar tooth extracted by a neighbouring farmer. The blood oozed from this for several days, (six) when I was called to him. I tried every other means without effect, and was obliged to cauterise it, which produced the desired effect.

Remarks by the Editors.

The records of medicine furnish us with some very remarkable instances of the kind related in the above authentic communication. In Huffeland's *Journal der Practischen Heilkunde*, for February, 1824, Dr. Elssaesser gives an account of a family in which the hæmorrhagic predisposition was strongly developed. This account states that *Jacob Sehr*, aged about thirty-four years, of a healthy habit of body, was married in 1816 to a young woman of a good constitution,

with whom he had had three male children at the time when this history was drawn up. Neither *Sehr* nor his wife, nor any of their parents or relations, had shown any extraordinary predisposition to hæmorrhage. In their three children however, this predisposition existed to a very uncommon extent. The first of these children appeared perfectly healthy when born. On the eighth day after birth, however, he sickened, and a hæmorrhage from the umbilicus came on, and continued in spite of every application that was made for its suppression, until he died on the fourteenth day. The second brother was born in 1818, and appeared, like the first, perfectly well. Dentition commenced in the tenth week, attended with the ordinary symptoms, diarrhœa and salivation. From this period the surface of his body was observed to be covered with a number of *dark* spots, from the eighth of an inch to an inch in diameter, and of irregular figure. The colour of these spots was at first reddish, often light blue, and finally of a light greenish red, and they depended evidently on extravasated blood. They disappeared generally in five or six days, and new ones on other parts of the skin came out, so that there were seldom less than twenty of these maculæ on the surface of the body. In the eighth month of his age he fell, and injured his mouth; a bleeding from the gums was the consequence of this fall, which lasted, with short intermissions, for seven days. Some time after he again fell, which caused a violent and long continued hæmorrhage from the nose. From the commencement of his second year, he was subject to frequent and alarming bleedings from the nose, which came on spontaneously. In 1821, he cut himself with a pair of scissors in the middle joint of one of his fingers, from which the blood flowed for eight days, notwithstanding every means that was resorted to in order to restrain it, and by which he was nearly destroyed. In 1822 a violent bleeding from the nose came on, in consequence of a fall, which, with occasional intervals, continued for nearly six weeks. The slightest wounds of the skin now gave rise to tedious bleed-

ings. In November of this year, he struck his head lightly against a hard body, by which a slight wound of the skin was produced; very little bleeding, however, came on, and the wound was soon covered with a thin crust. On the fifth of December, while playing, he pulled off this crust with his fingers; a very profuse bleeding immediately came on from the wound, and continued with unabated violence until he died on the following day.

The third son was born in 1821. He appeared to be in perfect health until he reached his eleventh week. About this time dentition commenced, and his body became covered, as in the preceding case, with dark blotches of extravasated blood. No hæmorrhage had as yet appeared. On the thirtieth of May, 1823, however, Dr. Elsaesser was call to visit him, and found him extremely exhausted from a hæmorrhage which came from a small wound he had received by knocking his head against a hard angular body. The blood in this instance, as well as in the preceding ones, did not issue in a stream as from a vessel, but oozed out copiously as from a sponge.

Professor Nasse of Bonn, in an interesting paper on hereditary predisposition to a fatal hæmorrhage, has collected a very considerable number of examples of this kind.*

ART. XI. *Two Cases of Carcinoma of the Ileum.* By GEORGE M'CLELLAN, M. D.

Case 1.—ON Thursday, the twenty-second of July last, I was requested to visit Mrs. M. A——, a delicate and interesting lady, who had, ten weeks before, been delivered of her third child. She was about twenty-three years old, and had enjoyed, with the exception of two severe puerperal attacks, an ordinary degree of health. Her first and third confine-

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* S. Archiv. f. Med. Erfahrung im Gebiet der pr. Med. Mai and Junius, 1820.

ments were premature,—occurring at the seventh month, and exhausting her system until the most alarming symptoms of debility and nervous agitation supervened. I was informed by her family, that, under these circumstances, she was supposed to be dangerously ill; and that she afterwards gradually recovered her strength and spirits under the use of tonics and antispasmodics. Her children, however, all survived, and appear to have attained the ordinary size and vigour. About seven weeks after her last confinement, when she had been restored to her natural strength and appetite, her husband sailed for Europe, on a visit to his friends. After that period, she was afflicted with a morbid vigilance and restlessness: she lost her natural composure of mind in the day time, and at night she experienced such constant uneasiness from fearful apprehensions and dreams, that her friends could not be induced to believe that she enjoyed any degree of sleep. A few days before I was sent for, she began to complain of dizziness, and of a peculiar sensation like sand rolling in her head. Her mind was also bewildered, on one occasion, with an extraordinary phantasm, in consequence of which she was perpetually disturbed with the fear of becoming insane. Her bowels had not been opened for some days, and she was occasionally troubled with colicky pains.

When I first saw her, she exhibited no signs of febrile action, either in the pulse or skin; but a great deal of irritation existed in her bowels and nervous system generally, as was evinced by a tenderness on pressure over the abdomen, and a constant restlessness and agitation of the muscles. I therefore depended upon the exhibition of purgatives, which were ineffectually administered, although they were followed by active injections. On the succeeding day, (Friday, July twenty-third,) her abdomen had become considerably distended, her pulse excited, and her face very much flushed. She was, therefore, bled to the extent of ℥viij in the morning, and of ℥xii more in the afternoon, at the same time that moderate doses of calomel were exhibited every other hour.

for the purpose of exciting catharsis. She fainted after both of these bleedings, and after the second, her pulse became extremely low and tremulous. Violent spasms also supervened, in consequence of which I was obliged to administer several doses of ether and assafœtida. At night, two assafœtida injections were thrown up without producing any effect on the bowels. At the same time a blister was applied to the nape, and sinapisms to the feet, for the purpose of removing the symptoms of cerebral irritation which were then present in an alarming degree. The next day, (Saturday) her abdomen had become almost tympanitic; and the muscles of her back and inferior extremities were afflicted with exceedingly painful cramps. An emetic of ipecacuanha and antimony was then exhibited, which not only produced a vomiting, but was soon after followed by copious evacuations from the bowels. This was the first purgative effect which could be induced by the action of any remedy in her case, and I can hardly avoid mentioning it as a strong corroboration of the views of Dr. Hosack respecting the favourable influence of emetics under such circumstances. Still, however, the symptoms of nervous irritation remained unmitigated; and in the evening her agitation became so severe, that I was forced to administer two and a-half grains of opium for the purpose of procuring sleep. To prevent the constringing effects of this remedy upon the intestinal, as well as the various other secretions, five grains of calomel were combined with it, in the form of a pill. The immediate effects of this prescription were very favourable. The patient, for the first time during nearly three weeks, slept soundly through the whole night, and in the succeeding morning she passed three large stools of a bilious appearance and consistence. At the same time the tympany subsided, and the tenderness of her abdomen became confined to a small space in the right iliac region. But the general irritation was not diminished:—on the contrary, her mind became more agitated, and the spasmodic contractions of her muscles more severe. My friends

Dr Monges and Dr. Eberle, were therefore called in consultation, and they afterwards regularly visited the patient with me, until the period of her death. As there was at first some difference of opinion between us respecting the seat and nature of the disease, and as the various symptoms which gave origin to the opposite opinions alluded to, may serve to elucidate the subject more clearly, I will detail the leading circumstances as particularly as may be consistent with the limits to which this communication must be confined.

I had all along acted on the opinion that the primary seat of Mrs. A——'s disease was in the abdomen, and the symptoms which I have already described as connected with that region, appeared to countenance this opinion. One of my friends, however, imagined that the description of the case corresponded exactly with the histories of some patients afflicted with *arachnitis*, as related by Drs. Duchatelet and Martinet, in their excellent work on that subject. He observed that Mrs. A—— expressed great surprise and astonishment in her countenance whenever any person entered the room, and even when she was addressed by any of her attendants or connexions on the most trivial subjects. She stared instead of looking at them. There was also a peculiar expression about her eyes whenever she attempted to raise the upper lids for the purpose of staring at any object;—instead of elevating the palpebræ alone in the usual manner, the eye-brows and whole forehead were drawn upwards at the same time, as if the levatores palpebrarum were so much weakened as to require the assistance of the occipito frontalis muscle in their action. There was also a great slowness of apprehension and indistinctness of speech; indeed, she could only utter monosyllables after the third day of my attendance. These symptoms have been described by Duchatelet and others, as characteristic of *arachnitis;* and as they occurred, in the present instance, in connexion with some more direct signs of cerebral irritation, the opinion did appear to be advanced with a strong colour of probability. My other friend, however,

conceived that the usual and most striking symptoms of meningeal inflammation, (such as quickness, hardness, and contraction of the pulse, heat and dryness of the skin, &c.) were not present; and that as there had been evident signs of abdominal disease, it would be more philosophical to attribute the cause to some derangement in that cavity. He agreed with me in suspecting that, notwithstanding so long a time had elapsed after parturition, there might be some connexion between the symptoms and her puerperal state; and also that the uterus might partake of the same irritation which probably existed in the peritoneal membrane. The symptoms of cerebral and nervous derangement appeared to him to be satisfactorily accounted for on the commonly received doctrine of sympathetic influence. Still, however, there prevailed no small degree of uncertainty in our minds, and we all proffered our opinions as mere suggestions or conjectures respecting an exceedingly obscure state of disease.

To allay the symptoms of irritation about the head, six ounces of blood were first drawn by cups, from the forehead and temples. No relief, however, was derived from this measure: on the contrary, the general agitation and spasms were greatly increased,—a cold sweat came over the whole surface,—and the pulse sunk into a low, fluttering condition. After the forces had been invigorated again by stimulants and antispasmodics, an infusion of senna and manna was resorted to for the purpose of clearing the bowels still more completely of their irritating contents. A large blister was also applied over the abdomen; and mucilaginous injections were occasionally thrown up the rectum. In the course of a few hours several bilious and mucous evacuations were procured by the action of the purgative, without relieving the tenderness in the right iliac region, or the general irritation of the nervous system. Opium was afterwards administered in large and repeated doses; but no sleep could be procured, nor was any respite afforded to the spasms. Notwithstanding all the various antispasmodic articles which were employed, the symp-

toms continued in nearly the same condition until Wednesday, July twenty-eighth, when severe convulsions of the whole muscular system began to appear. These convulsions sometimes alternated with a slight coma, at other times they left the system in a more tranquil condition than had been witnessed for several days before. They subsequently increased in frequency and intensity, until Friday, the thirtieth of the same month, when the patient expired, on the ninth day of my attendance.

We examined the body eighteen hours after death, and the following circumstances were disclosed.—No trace of disease, whatever, could be detected within the cranium, either in the brain or its membranes. The vessels of the pia mater did not appear more turgid than natural, nor was there an unusual quantity of fluid collected in the ventricles. We were all equally surprised at these circumstances; for whatever difference of opinion had occurred amongst us in the early stage of the disease, the appearance of convulsions induced an unanimous belief that the encephalon was affected.

We next laid open the abdomen, and at first could discover no sign of morbid action in that cavity. The peritoneum appeared perfectly smooth, and free from every trace of adhesion and morbid secretions. The viscera presented no vestige of inflammation, until on inspecting the lower portion of the *ileum*, a few discoloured spots were observed in the parietes of that intestine. These spots were of a reddish-brown or livid colour; and they were placed at short distances (from one to three inches) apart from each other. They were of various sizes, from one-quarter to two-thirds of an inch in diameter; and they altogether occupied about the last foot of the length of the ileum. On a close examination they were found to be considerably thicker and harder than the surrounding portions of the intestine; and the blood-vessels which extended from them were greatly dilated and distended with blood through the whole breadth of the mesentery. The mesenteric glands which were situated along the course

of these dilated vessels, were also found to be enlarged and converted into a soft brain-like structure. Several of them were as large as a hickory-nut, and they all presented the characteristics of *medullary sarcoma* so strongly, that we did not hesitate to rank them under that name. On laying open the intestine, the diseased spots in its parietes were found to be prominent internally, with whitish jagged ulcers on their surfaces. But the principal disease existed at the very termination of the ileum, about three and a-half inches of which were greatly thickened, contracted, and ulcerated. The cavity of this portion of the intestine was almost entirely obliterated, and it appeared quite surprising that any evacuation could have been procured from above. The thickened parietes of this diseased part were indurated, and almost cartilaginous in texture. Internally the surface was cracked or ulcerated into deep and irregular fissures. Altogether the whole mass very closely resembled one of the forms of cancerous lip, a fatal case of which lately occurred to me, presenting precisely the same kind of disorganization in the lymphatic glands of the throat, as was observed in the mesenteric glands of this individual.

The only morbid appearance that we could discover in any other part was in the uterus, the structure of which was friable and somewhat granulated. On laying it open, its parietes could be torn very easily in every direction. This circumstance struck us as a remarkable one, when we considered how recently parturition had taken place, and how easily a rupture of the organ might have occurred under the slightest efforts at contraction.

The integuments were remarkably thin and transparent, allowing the subjacent veins to be seen on every part of the surface; and the whole appearance was such as would generally be considered to denote a predisposition to scrofula.

Case 2.—The second instance, which I believe to be an incipient case of the same disease, occurred in the person of a little girl nineteen months old, daughter of the Rev. M. M.

Carll, of this city. She had always been of a pale, leucophlegmatic habit; and during the earlier part of the past season was afflicted with a tedious and harrassing cholera, which did not subside until she was carried into the country for the benefit of fresh air. While she remained there, for several weeks afterwards, the disease was perfectly relieved; but on the very next day after returning to the city, it re-appeared with great violence. Blood was discharged in considerable quantities along with the stools, and her strength rapidly declined. By the action of two grains of calomel, followed by small and frequently repeated doses of antimonial solution, these unnatural discharges were very soon corrected; but she was left cold and extremely exhausted during the succeeding night. On the next morning I found her labouring under symptoms of severe nervous irritation, almost amounting to convulsions. Her eyes were distorted, her respiration was irregular, and the whole muscular system appeared to be exceedingly agitated.

There was also a distressed sensation about the throat, which occasionally produced a spasmodic attempt at swallowing. As I had before known these symptoms to originate from the presence of worms in the stomach, I did not hesitate to ascribe them to the same cause on this occasion. After administering five grains of calomel, I, therefore, prescribed fifteen drops of the spirits of turpentine, to be repeated every third hour. But in less than an hour afterwards, I was again sent for in great haste, with a notice that the child had fallen into convulsions almost immediately after taking the dose of turpentine. Before I arrived she had expired in one of these convulsions.

On examination of the body six hours after death, I was altogether disappointed in my search after intestinal worms. All of the glandular viscera presented a healthy appearance. The stomach and intestines were found to be perfectly empty, pale, and free from every appearance of inflammation. The lower portion of the ileum, however, was studded over with

nine livid spots, from each of which enlarged veins could be traced deep into the mesentery. Along the course of these veins the mesenteric glands were evidently diseased,—presenting a brown or bluish colour, and a softened structure. They were mostly of the size of filbert nuts, and yielded a mucous fluid when compressed between the fingers. On laying open the cavity of the intestine, the discoloured spots were found to correspond to internal tumours of the mucous coat, of a flat, circumscribed appearance, and oblong figure. These tumours were generally about three inches in circumference; and four of them were superficially ulcerated in the centre, so as to resemble very closely the appearance of chancres on the prepuce. Around the base of each tumour were situated a number of very small white tubercles. Both portions of the fallopian valve were thickened and ulcerated, and from the inner surface of the colon, a thick lining of coagulated lymph could be scraped off with a knife.

I trust that the preceding cases will not be found altogether uninteresting to any of our readers. They will serve, at least, to show the value of *post mortem* examinations in explaining such causes of death as cannot otherwise be ascertained; and they will probably excite some attention to the too much neglected subject of specific diseases. From my own experience I have long been convinced that carcinomatous and other morbid structures, occur much more frequently than is generally imagined. They are not less liable to originate among the internal parts than upon those external organs where alone they are usually discovered by practitioners. In examining the bodies of such patients as have died of obscure and undetermined diseases, I have frequently discovered internal derangements, which could not have been suspected from a previous attention to the symptoms. Whether those which I have now described can be ranked as really carcinomatous or not, it is hardly necessary for me to consider, for that appellation has hitherto been so vaguely applied as to designate various kinds of morbid structure.

ANALECTA.

The following remarks upon wearing Flannel, are extracted from a late excellent work upon Pulmonary Consumption, by Dr. Armstrong, of London, a gentleman distinguished equally by his love of science, and his ardent zeal for the progress of liberty in this and all other countries.

"When flannel is worn next the skin, Dr. Barlow remarks, it is almost invariably the practice to keep it on by night as well as by day. This is not only unnecessary, but injurious. The chief advantage of using a flannel dress next the skin, results, not from the actual warmth imparted or retained, an effect which might be obtained to an equal extent by an increase of outward clothing,—but from the uniformity of temperature thus insured to so large a portion of the surface of the body, and the tendency which it has to keep the highly important, but too much neglected, functions of the skin in an active and healthy condition. During the day, the frequent and oftentimes sudden vicissitudes of our climate, are such as to render the effects of flannel in preserving an equality of temperature, most valuable. But at night, and during sleep, we are exposed to no such vicissitudes; consequently, the same necessity for the use of flannel does not then exist. But while the use of flannel at night thus appears to be unnecessary, there are several considerations that show it to be injurious. These considerations regard both the condition of the body, and of the flannel itself. Whatever the wants of the body for warmth during the night may be, they are, in general, fully supplied by the bed-clothes in common use. The body requires no additional warmth during sleep: on the contrary, there is at such times even a tendency to an increase of the natural warmth. When to this natural tendency the heat caused by flannel next the skin is superadded, the effect is to keep the skin in a state of considerable excitement, and to induce perspiration more or less profuse. These effects are not calculated to prepare the body for enduring the vicissitudes of the ensuing day, but rather to render it more susceptible of injury. Again, continues Dr. Barlow, the property which renders woollen cloth so eminently suited to the purposes in view, is that of its being a slow conductor of heat. This property is directly proportionate to its dryness, and is greatly impaired by its imbibing humidity of any kind. A flannel dress, however, that is worn next the skin throughout the night, becomes so charged with perspiration, that its power of conducting heat is thereby greatly increased, and its preservative effects proportionably diminished. Here, then, is a two-fold injury resulting from the prevailing practice of continuing the flannel dress during sleep: namely, a diminution of the preservative powers of the flannel, and an increased susceptibility of the skin. By laying

aside the flannel dress on going to bed, and substituting one of coarse calico, the body is kept in that temperature during the night, which fits it for encountering the vicissitudes of the following day, while the flannel is preserved from the deteriorating effects of the nightly perspiration, and is resumed in the morning in a state which contributes both to comfort and protection. A difficulty, Dr. B. subjoins, is experienced with most people who have accustomed themselves to the nightly use of flannel in inducing them to alter the habit. Fear of taking cold creates one great obstacle: and a disinclination to the feeling of cold experienced at the moment of changing the flannel for the calico night-dress, especially in winter seasons, is another. Confidence in the medical adviser, however, is sufficient to remove the first, and a very little experience to remove the latter; for after a very few trials, the proposed change is found to prove a decided gain, even on the score of sensation, and the gratification derived from resuming a dry and comfortable flannel in the morning, together with the sensible increase of its utility during the day, are found to compensate amply for the slight unpleasantness attending the momentary exposure of the preceding night.

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We abstract the following very interesting article from the London Medical and Physical Journal for May 1824:

Hyalonyxis.—This term is derived from *υαλος, glass,* and *νυσσω, I pierce;* and is applied by Dr. John Brewer to a new operation proposed by him for the removal of every species and variety of cataract. Dr. B. states, in his work on this subject, that he has performed this operation upwards of one hundred and sixty times during the last four years of his residence in Italy. He also assures us, that thirty of thirty-one patients have been restored to sight by this mode of operating. Inflammation is of rare occurrence, provided proper precautions are used.

The operation is thus performed, three or four hours previous to which the patient's eye-brows should be rubbed with some of the extract of belladonna, rendered soft by the addition of a little boiling water:

"The patient being seated, so that the superior part of the head does not reach higher than the superior part of the sternum of the operator, he takes his needle, previously besmeared with oil, as a pencil or writing-pen, placing the weight of his hand on the cheek of the patient: this produces two very considerable advantages; first, it steadies the hand; second, in the event of the patient's moving, the hand preserves its relative position with respect to the eye, so that any movement of the patient's head can be of no importance to the surgeon. The patient being directed to turn the eye towards the nose, the needle, with its convex

surface* forwards, corresponding with the iris, is boldly introduced into the globe of the eye, through the sclerotic, three lines or three and a-half from the transparent cornea, and a line below the transverse diameter of the pupil, to avoid wounding the ciliary artery, which pursues its course to the iris between the sclerotic and choroid coats, along the middle of the external convexity of the eyeball. If introduced at or below this point, you avoid all effusion of blood. The instrument passes into the vitreous humour, posterior to the lens and its capsules: the point of the needle is then brought forwards, from inclining the hand to the temple, and penetrates the posterior capsule.

"Should the lens be soft or fluid, the needle passes through its centre on its passage to the anterior chamber; if solid, the instrument should be directed to its superior part, and by gently elevating the handle, the lens will be sufficiently depressed to allow the instrument to pass between it and the ciliary processes; it is then carried through the anterior capsule and pupil, (which latter has been previously dilated with the belladonna,) into the anterior chamber, without any risk or danger of wounding either ciliary ligament, iris, or ciliary processes. The object the operator has in view, is the laceration and removal of a large portion of the anterior capsule, that is to say, of greater extent and diameter than the pupil, in its natural, most dilated state. This is effected by three or four circular movements of the point of the needle, which should invariably be done. If the capsule be transparent, the inexperienced operator will scarcely be sensible of effecting any good or change by this circular movement of the needle.

"The correspondence in transparency and colour to the aqueous humour is so similar, and the former so delicate, that no resistance to the needle is visible to the fingers, when the point is breaking down and removing from the axis of vision the membrane, so frequently the cause of secondary cataract. When the capsule is opaque, the effects of the needle are of course evident: the transparent or opaque portions should be conducted backwards or downwards with the lens, and buried in the vitreous humour, below the margin of the iris, and as much as possible removed from the pupil; on retiring and withdrawing your instrument, a circular movement is similarly made with the point, for the purpose of removing every portion of the posterior capsule, which is also liable to opacity; so that a communication is completely established between the aqueous and vitreous humours. The anterior capsule, lens, and posterior capsule, are now re-

* To avoid still more the wounding of the ciliary nerve or artery, the needle may be introduced with its cutting edge corresponding with the iris; and after having entered the eye, to be turned round with its convex surface forwards, and continued as we have before described.

moved; so that the utter and total impossibility of secondary membranous cataract forming, must be evident.

"Such is the operation for a solid cataract enveloped by its capsule; but the lens we meet with, at least seven in ten cases, is either fluid, semisolid, or curdy. When the instrument, therefore, penetrates the anterior capsule, and the operator commences his delicate circular movement, the aqueous humour becomes cloudy or discoloured, which is the passage of parts of the diseased lens into the anterior chamber, where its absorption is more rapid than in the vitreous humour.

"The views of the operator are the same: instead of quickly finishing the operation, the solid particles of the lens should be removed by several movements of the needle as much as possible from the axis of vision, either by depressing them into the vitreous humour, or bringing them into the anterior chamber. The free movements of the needle. and removal of the capsules into the aqueous or vitreous humours, as we have above described, ensure the success of the operation.

"The great desideratum in the fluid cataract, is the laceration and removal of the capsules from the axis of vision; for in all cases where a free communication is once established between the aqueous and vitreous humours, absorption goes on, and your cure is almost certain.

"We have now described the removal of the lens in two different states: there only remain some observations on the third.

"I have occasionally met with the lens united firmly to both capsules, forming, as it were, only one body. These cataracts, from their size and appearance, are easily distinguished from what Scarpa very properly termed primary membranous cataract, which is the wasting of the chrystaline without its capsule, which appears contracted and more opaque in its centre than common cataracts. The most correct account, however, we have of this variety, is from the late Mr. Saunders of London, published by his friend and colleague, Dr. Farre, an eminent physician. These cataracts are, for the most part, congenital; and although Scarpa represents them as rare, are not uncommonly met with in children. When the lens in these cases is absorbed, the anterior lamella of the capsule, unites itself to the posterior, until they form one membrane, which is white, opaque, and elastic: this membrane is absorbed, after puncture, with the needle and partial laceration. A most interesting account is given by Mr. Saunders, with their removal in children."

On the employment of Oil of Turpentine in Sciatic and other Neuralgiæ—M. L. Martinet relates that of thirty-six individuals labouring under acute as well as chronic neuralgia, twenty-six were completely cured, seven experienced considerable relief, and in three only, one of whom was affected with a mortal hip

disease, did the treatment by the oil of turpentine fail. The use of the remedy produced but trifling inconvenience, and the common duration of the treatment was only six days.—*(Revue Med.) London Med. Repository.*

Use of Sulphate of Copper in Croup.—Dr. H. Hoffman recommends the sulphate of copper as an excellent remedy in croup, especially after blood-letting. In slight cases he begins with giving from a quarter to half a grain every two hours; in those cases, however, where there is also laryngitis, or bronchitis, three, four, or more grains are administered, so as to excite instant vomiting; by so doing, the Dr. thinks that not only is the lymph expelled from the trachea, but also that the further secretion of it is prevented, so that the patient is very much relieved, and soon cured. After copious vomiting has been produced, the medicine is to be given in small doses, in conjunction with digitalis. In support of the utility of the above practice, Dr. H. affirms that he has employed it with the greatest success during a period of ten years, in a great number of children affected with croup, without ever having lost a patient in that time, notwithstanding the disease was often at its height when he was first called in.—*Med. Repository.*

New Remedy for Tape-worm.

Brayera Anthelmintica.—Dr. Brayer, who resided a long time at Constantinople, has brought with him a new remedy for tape-worm, which is said to be very valuable. It is an Abyssinian plant, now much employed as a vermifuge throughout the whole Turkish empire. He became acquainted with it in the following manner:—He frequently met with an old Armenian merchant at a coffee-house in Constantinople, who gave him an account of his journies to Cairo and Abyssinia, and particularly of the medicinal powers of the plants which grow in that country. Among other things he observed, whilst looking at the principal waiter of the coffee-house, who was troubled with a tape-worm, and who had employed the most eminent Turkish physicians without receiving the least benefit,—that man, he said, were he in Abyssinia, would be relieved of his worm in twenty-four hours. He, at the same time, told Dr. Brayer, that he had written to his son, then travelling in Abyssinia, to send him some of the flowers of the plant *kotz*, as it is called in that country, and which he stated was the remedy employed by the Abyssinians for tape-worm. Dr. Brayer paid but little regard to this information, at that time; but his attention was particularly drawn to it, when, in January 1822, the same coffee-house waiter came to him in an unusually lively mood, and said he was cured. The flowers sent for

by the old merchant, arrived on the fifth of January. On the following morning, the patient drank, on an empty stomach, a half pint of an infusion made with five drachms of the flowers to one pint of water; in two hours after he took another half pint. Soon after the second dose was taken, he felt violent pains in the abdomen, and in a short time the whole dead worm was expelled.

According to some specimens brought back, Kunth has ascertained that the plant belongs to the *rosaceæ*, approaching in its character to *Agrimony*, and to which he has give the name *Brayera Anthelmintica*. This plant is a small tree, with branched, hairy, alternate leaves. The only difference between the flowers of the Brayera and Agrimonia, is that the margin of the calix of the former is double, and the petals very small and more separated.*

Nouveau Journal de Medecine, August, 1822.

Vapour Bath in the Hardening of the Cellular Tissue of new-born Infants.—This affection, which the French call *endurcissment du tissu cellulaire*, still prevails every winter, almost epidemically in the Foundling Hospital of Paris. So fatal is this disease, that out of six hundred and forty-three infants who were affected with it from the year 1808 to 1811, only sixty-seven recovered. It was observed that the disease was much less frequent and mortal when the house was furnished with a sufficient number of wet-nurses, than when the infants were nourished by artificial diet. After many unsuccessful experiments, a remedy has at last been discovered, which has hitherto been found to prove very successful. This remedy is the *vapour bath*. The infants are immersed in an aqueous vapour bath of the temperature of about 96°. From six to eight baths are in general sufficient to effect a cure.

New mode of operating for Sarcocele.—Mr. Humont, an eminent military surgeon, in order to avoid the extensive wound, hæmorrhage, and subsequent profuse suppuration which are frequently unavoidable in the ordinary mode of operating for sarcocele, has performed this operation in the following manner, with the most favourable result. The patient was placed on his back, with the pelvis considerably elevated, and the thighs separated. The operator, standing on the side of the affected part, grasped the tumour with one hand, raised it up and drew it forwards over the *os pubis* and groin of the opposite side, so that the lower part of the tumour presented above, and the posterior part forwards. Taking a bistoury, as if preparing to cut from without inwards, he made an

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* Archives Genérales de Medecine,—Janvier, 1821, p. 116.

incision from above downwards three and a-half inches in length, extending to the inguinal ring. Having divided the cellular layers of the dartos, the diseased part came to view between the lips of the wound, which were separated by an assistant. The tumour was now separated from its surrounding adhesions, and the spermatic cord insulated. After the inguinal ring was sufficiently exposed, the cord was divided with one stroke of the knife. The blood spouted from four arterial branches, which alone were tied. The artery of the septum scroti being uninjured, was easily distinguished by its pulsations, running along the side of the incision. The wound was united by adhesive straps, and healed with but little suppuration. This mode of operating is not practicable in all cases; but in cases where the surgeon can select the part for his incision, this method appears to be preferable.

Poisoning of a Suckling Child by the milk of the mother.—W. Tourtnal relates the following remarkable circumstance:—A married man quarrelled with a soldier in the presence of his wife. The soldier drew his sword and made an attack upon the husband; in consequence of which the wife was thrown into the utmost terror. She immediately afterwards put her eleven months old child to her breast. In a few minutes the child ceased to suck, became slightly convulsed, and immediately died.

Hufeland's Journal, February, 1823.

Account of an Essential Oil, which flows spontaneously from a Tree in South America. Communicated by Dr. Hooker.

It is with much pleasure we are enabled to lay before our readers some account of a fluid, containing all the properties of an essential oil, which the natives of South America, in the district between the rivers Parime and Oronooko, are in the habit of extracting from a tree which, though it abounds in those regions, is unfortunately, as yet, unknown to naturalists. The information has been communicated to us by our excellent and scientific friend, C. S. Parker, Esq of Glasgow, who, in a late visit that he made to Demerara, suffered no opportunity to escape him of investigating the botany of those fertile regions, whence he has already transmitted to his native country several valuable vegetable productions.

Mr. Parker's attention was particularly attracted by the *Native Oil of Laurel*, a fluid which seems to have been but recently heard of in the Dutch settlements, and which we are assured by Mr. Edmondstone of Cardross Park, near Dumbarton, (whose long residence in that country, as Protector of the Indians, gave him unusual facilities in obtaining scientific information,) was also unknown to those people a few years ago. Aided by the

previous attention that Dr. Hancock, an experienced physician in the colony, had paid to the nature of this oil, Mr. Parker drew up an account of it, which was inserted in the Colonist (Demerara) newspaper; Jan. 18, 1824, and which, as comprising every thing that was known upon the subject up to that period, we shall here insert.

"*To the Editor of the Colonist.*

"Sir,—I take the liberty of communicating, for insertion in your Journal, a few observations on a very extraordinary vegetable production, the knowledge of which has, hitherto, been almost exclusively confined to the natives of Spanish Guiana. This substance, which has very injudiciously been termed *Azeytode Sassafras,* (an appellation which tends to confound it with the essential oil yielded by the *Laurus Sassafras* of the Northern Continent of America,) affords, so far as my knowledge extends, a solitary instance of a perfect volatile liquid, without the aid of art. Substituting for the appellation to which I have objected, the provincial name of '*Native Oil of Laurel,*' I shall describe the method of procuring it, and enumerate its principal chemical and medicinal properties, so far as these have been investigated and examined.

"The *Native Oil* is yielded by a tree of considerable height; its wood is aromatic, compact in its texture, and of a brownish colour, and its roots abound with essential oil.

"This tree, which is found in the vast forests that cover the flat and fertile regions between the Oronooko and the Parime, has, from an analogy already alluded to, been supposed to belong to the natural order *Laurineæ;* and though Humboldt and Bonpland do not seem to have been acquainted with its singular and important produce, its botanical characters may very possibly have been described in their *Nova Genera et Species Plantarum Americæ Septentrionalis,* under the genera *Ocotea*, *Persea,* or *Litsea.* This question I am, however, unable to solve, having never seen the parts of fructification.

"The *Native Oil of Laurel* is procured by striking with an axe the proper vessels in the internal layers of bark; while a calabash is held to receive the fluid, which gushes out in such abundance, that several quarts may be caught from a single incision, if the operation be performed with dexterity. So obscure, however, are the indications of these reservoirs, that the Indians, (with perhaps a little of their usual exaggeration) assert that a person unacquainted with the art may hew down a hundred trees, without collecting a drop of the precious fluid. In many of its properties the native oil resembles the essential oil, obtained by expression, distillation, and other artificial processes: it is, however, more volatile and highly rectified than any of them, its specific gravity hardly exceeding that of alcohol. When pure, it is colourless and transparent; its taste is warm and pungent; its odour aromatic, and closely allied to that of the

oily and resinous juice of the *Coniferæ*. It is volatile, and evaporates without residuum at the ordinary atmospheric temperature. It is inflammable, and, except when mixed with alcohol, gives out in its combustion a dense smoke. Neither the alkalies nor the acids seem to exert any sensible action upon the *Native Oil;* when combined, however, with sulphuric acid, the mixture assumes a momentary brownish tinge, but soon regains its transparency. The Oil of Laurel dissolves camphor, caoutchouc, wax, and resins, and readily combines with the volatile and fixed oils. It is insoluble in water, soluble in alcohol and in ether. Though the specific gravity of the oil greatly exceeds that of ether, the compound, formed by combining them in the proportion of one part of the former to two of the latter, floats upon the surface of pure ether, and may therefore be the lightest of all known liquids.

"With respect to the medicinal properties of the *Native Oil*, it bears, when externally applied, the character of a powerful discutient, and appears, when exhibited internally, to be diaphoretic, diuretic, and resolvant: by many it is believed to be analeptic, alterative, and anodyne; and to promote the exfoliation of carious bones.

"Without listening to the extravagant reports of the Indians, who exalt it into a panacea, we must admit that its efficacy has been demonstrated in cases of rheumatism, swellings of the joints, cold tumours, and in the various disorders supposed to originate in a vitiated state of the blood (*mala sanguis.*) In all these cases it is exhibited in doses of from thirty to forty drops, twice a-day, accompanied by frequent and continued friction of the parts affected with the oil, while the body is kept moderately warm, and a free use of diet and drinks prescribed to the patient. The same practice is said to have been attended with the happiest effect in paralytic disorders; for this I cannot vouch, but have found it a valuable remedy in cases of nervous rheumatic headache, sprains, and bruises. A decoction of the root has been recommended as an alterative in various chronic complaints.

"I am fully aware of the reaction that often results from over-excited and disappointed expectation, and of the discredit into which a new remedy frequently falls in consequence of the unmerited encomiums which those who bring it into notice have injudiciously bestowed upon its virtues.

> 'Quidquid excessit modum,
> Pendet instabili loco.'

"However slight the credibility we may feel inclined to attach to the evidence of the Indians, upon which our knowledge of the medicinal properties of the native oil almost entirely reposes, the information derived from experience surely claims that attention, and justly challenges that examination, which we should not hesitate to bestow on the speculations of the mere theorist

Let inquiries be instituted, and experiments be made, by those whom their situation and scientific attainments qualify for the task. By these investigations it may not only be ascertained what degree of confidence ought to be reposed in the unqualified encomiums which the Indians lavish upon this anomalous production, but properties unknown to them may be discovered; and its history, which they have been accused, perhaps unjustly, of involving in obscurity, be satisfactorily elucidated. To the chemist, and the vegetable physiologist in particular, the *Native Oil of Laurel*, elaborated by the unassisted hand of nature, in a state of purity which the operose processes of art may equal, but cannot surpass, presents an interesting subject of inquiry, and a wide field of speculation."

Samples of this oil having been received by H. Rainy, Esq. they were, through his liberality, communicated to Dr. Thompson Dr. Wollaston, and Mr. Macintosh. The latter gentleman has found it to answer the purpose of dissolving the caoutchouc, preparatory to this substance being employed to render cloth, &c. water-proof, equally well with the Naphtha which he has been accustomed to use. It is therefore to be hoped, that besides its medical properties, the Native Oil of Laurel may be found to possess qualities that may be serviceable in the arts, and thus become an important article in commerce.

We may here observe, that in M. Kunth's Synopsis of the plants described in the *Nova Genera, &c.* of Humboldt, mention is made of a doubtful species of laurel, *Laurus Jaritensis*, (which is not, however, enumerated in the large work,) a native of shady forests in the district of Oronooko, of which it is said, "Folia odorem terebinthinum spirant." May not this be the tree in question? The great South American travellers, Humboldt and Bonpland, did not, however, discover it either in flower or fruit.

We are assured by Mr. Parker, that Dr. Hancock intends to use his utmost endeavours to procure perfect specimens of the plant which affords the *Native Oil of Laurel*, which he will transmit to this country.

Dreadful Effects of an Excessive Use of Sulphur.—Professor Olmsted, in a geological excursion in the county of Wake, North Carolina, met with the following fact, which we present in his own words: "At Mrs. Thomson's, where I dined, I saw a fellow creature whose sufferings made me truly thankful even for my own imperfect health. He was the son of Mrs. T., and nearly fifty years of age. When I came into the porch, he was sitting before the entrance in an elbow chair, surrounded with pillows, with no clothing but a frock of linen, that came half way down his knees. The ghastly image of death was imprinted on every part of his emaciated frame. The bones of his arms and legs were hardly covered with flesh, and the joints of the knees, and

the largest joints of the fingers, were increased to an enormous size. His knees were drawn together beyond the power of separation, shortening the left leg so that nothing but the toes reached the floor. The hands were forcibly and irremovably fixed to the breast, the distorted fingers being bent backwards, forming an arch over the swollen mound at the joints. A little motion with the ends of the fingers was all the sufferer could produce; and I was much impressed with the insensibility of man to his ordinary blessings, when I saw this poor invalid call several times for a servant to come and brush off a fly that was biting his leg unmolested. On his back was a large running sore, which his friends were uncertain whether to ascribe to disease, or to the effects of lying so long in a horizontal and immovable posture. The daily dressing of this sore, which was performed while I was present, gave him so great pain, that he was unable to sit up a moment after it was completed. Three servants took him up with all possible care, and laid him on the bed; while the excruciating misery which this gentle movement produced, filled the mouth of the sufferer with groans and entreaties. Here he lay, bewailing his lot in a hollow, piteous tone, and crying, "Oh that thou wouldst hide me in the grave, that thou wouldst keep me secret, until thy wrath be past!" On inquiring the cause of his sufferings, I was told that, three years ago, being afflicted with rheumatism, he took the advice of a quack to attempt its cure by enormous doses of sulphur. With this view, he mixed a pound of sulphur with five quarts of water, and stirring it, took half a pint three times a day, until he had taken six pounds of sulphur. Soon after, commenced the pain and distortion of his limbs, which had subjected him to increasing and excruciating sufferings ever since."—*Silliman's Journal.*

Medicinal properties of the waters of the Mississippi—Mineral impregnation of the well of Henderson, Ohio. [Extract of a letter to the Editor.]—"My health being much enfeebled, necessity compelled me to take a trip to New Orleans for the sake of using the water of the Mississippi river. The effect was salutary and the voyage a pleasant one. The Mississippi water, when freely drank, produces powerful effects—when filtrated it is very clear, and may be drunk in large quantities without burthening the stomach. It certainly possesses some properties not common to other waters, but its qualities I believe have never been examined by chemists. My ill health I attributed to the water of Henderson. It is strongly impregnated with some mineral. The common opinion is, that it is impregnated with copperas. The ground on which Henderson and the country adjacent stand, has, from 40 to 60 feet beneath the surface, a substratum of mineral coal, the best for fuel and the forge that I ever saw. This coal is washed bare on the banks of the Ohio and Green rivers—intermixed is a

mineral of a bright green and yellow hue, which is used for the same purposes as copperas. Families that die woollen or cotton cloth collect a quantity of this coal, put it into a proper vessel, to which they add the water, and boil till the water has extracted the mineral from the coal. In this water they immerse the articles to be dyed. Copperas too has been manufactured from this coal, in some small quantities, and sent abroad for market.

"Through this coal all the waters in and about Henderson probably pass, and thereby acquire strong mineral properties, which give them a particular taste, and render them pernicious to health."—*Silliman's Journal.*

Tartar Emetic—in Pneumonic Affections.—Mr. Pechier, physician at Geneva, states that he has found by experience, that tartar emetic is an exceedingly efficacious remedy in pneumonic inflammation. He was first led to use this article in cases of this kind, from the following considerations: As an evacuent, tartar emetic must cleanse the stomach and bowels, promote the circulation in the portal system, diminish the mass of blood circulating in the thoracic viscera, and, by weakening for a time the digestive powers of the stomach, check chylification and the too rapid formation of blood. Besides these effects, it appeared to him, that the disturbance produced by vomiting, in the animal economy, must tend to divert from the breast those defluxions (*le mouvement fluxionaire*) which occur in pectoral complaints. He states that in all cases of pleurisy, (*le point*) in which he was consulted, he immediately resorted to the use of tartar emetic. He gave it at all periods of the disease, and under every circumstance, under which it occurred to him. He generally gave from 6—12 to 15 grains in twenty-four hours, dissolved in about six ounces of water, in doses of a table-spoonful of the solution, every two hours. In addition to this remedy, he directed the patient to drink every hour a cup of some mild aperient infusion; and if the patient experienced much distress and anxiety of feeling, he ordered a full dose of laudanum to be administered. When dysury with a dry hot skin came on, he directed from one to two drachms of nitre to be added to the solution. He noticed the following effects from the influence of this remedy: Generally after the second or third dose, the patient vomited, which was shortly after followed by one or two evacuations from the bowels. Sometimes it produced no sensible effect, although the patient perceptibly grew better. He generally found the patients to experience a sensation as if a piece of velvet were applied to the skin of the breast. If before the disease was subdued the remedy was omitted only for 3 or 4 hours, the symptoms invariably became worse. He says that his experience has taught him that tartar emetic given in large doses is much less apt to excite vomiting, than when the doses are small.

He generally cured his patients with eight doses of the medicine, and seldom found the disease to continue beyond the third week. In addition to the employment of the tartar emetic, he, in some instances, applied a blister over the affected part.—*Bibliotheque Universelle, Juni p.* 144. *See also Annales Cliniques de Montpellier, T. XLII. p.* 171.

Hydrops Soccatus Ovarii.—Dr. Kruiger-Hansen, of Guistraue, treated a case of *Hydrops Saccatus Ovarii* successfully, in the following manner: After tapping it, he inflated the sack through the canula of the trochar, and suffered the air to remain in it 15 minutes. The patient felt herself much relieved by this operation. After a few months the ovarium enlarged again, and the part which had been punctured by the trochar inflamed, broke open, and discharged for several days a purulent fluid, followed by the discharge of a great number of small pieces of membrane having a putrid smell. She did not appear to suffer much in her general health during these discharges; the wound gradually closed, and the woman is now well.* Dr. D. Zandi, professor of Surgery in the university of Halle, in a volume published in 1816,† gives an account of several cases of encysted tumor treated successfully in the same way. In his interesting essay on this subject, he maintains that encysted dropsy, both of the peritoneum and ovaries, may be cured by a surgical operation. He was led to adopt this practice by the following incident: A lady at Halle, had a large encysted tumor on the crown of her head, which had been of several years' standing. A piece of lunar caustic was laid on the middle of the tumor, and kept on by an adhesive plaster. The caustic formed an opening into the tumor and gave discharge to a matter resembling a thick solution of gum arabic. A compress was then laid on the opening, and secured by a deachylon plaster. The matter continued to flow from the wound for several days, and became towards the end very fetid. In about fourteen days after, the internal membrane of the cyst began to separate along the edges of the orifice, and soon became entirely separated from its surrounding parts. The cyst was then drawn out by degrees through the opening with a small forceps. The wound healed rapidly, and the patient was rid of her tumor. He also gives an account of an abdominal encysted tumor which was treated with success in this way. The whole cyst was gradually drawn out by the wound. He advises, that after the fluid is evacuated, a long tent be introduced through the orifice into the cyst. A slow inflammation or separation of the cyst supervenes, and may then be extracted by the surgeon.

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* Graefe and Walter's Journal, III—4.

† Beitraege zur vervollkommung der Heilkunde, Halle, 1816.

On the Internal use of Muriate of Ammoniæ; by Dr. A. F. Fisher.—Dr. Fisher recommends the internal use of large doses of sal amoniac as a most valuable remedy in schirrus of the prostate gland; and in thickening and even ulceration of the internal membrane of the bladder. It should be given in very large doses, and used for a considerable time, in order to obtain its beneficial effects in these affections. When properly used, it seldom fails to perform a cure. Dr. F. asserts, also, that this remedy has afforded the most unequivocal benefits in schirrus ovaria and uterus. Drs. Blume and Kuntzmann, have published statements in confirmation of the experience of Dr. F. on this subject. They speak of this article as a very powerful remedy in the diseases mentioned. Dr. F. gave it in combination with bitter extracts.—*Hufeland's Journal, Feb.* 1824.

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Artificial Castor Oil.—Dr. Hufeland states that a drop of the *oil of croton* added to an ounce of *Ol. papaveris,* forms a mixture exceedingly similar to Castor Oil. A table-spoonful of such a mixture produces the same effect as the same quantity of Ol. Ricini.—*Hufeland s Journal, Feb.* 1824.

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Muriatic Acid generated in the Stomach during the process of digestion.—It has long been known to physiologists that a powerful acid exists in the stomach of animals during the digestive process. Many experiments have been performed by different chemists to ascertain the nature of this acid, and they have generally led to the conclusion, that it was the acetic. The experiments of Dr. Prout, however, which were lately communicated by him to the Royal Society, prove, what he had before suggested,—that the saline matters found in the stomach consist chiefly of the alkaline muriates, and also a large portion of *free muriatic acid.*

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Injection of Opium into the Veins.—Mr. Coindet, of Geneva, conceiving that opium might be rendered inert by the influence of digestion upon it while in the stomach, injected it into the veins with remarkable success in a case of idiopathic tetanus. The patient was a young girl, 14 years old, who had been subject to the most violent form of hysteria, the paroxysms, of which finally became exasperated so violently as to assume the appearance of tetanus. In these paroxysms, which sometimes lasted twenty minutes or more, the whole muscles of the body participated in the painful tension. The respiration was embarrassed —the pulsations of the heart became enfeebled and irregular— and the poor girl was threatened with suffocation. This succes-

sion of paroxysms had lasted three weeks, and was increasing in degree, when Mr. Coindet visited the patient. Her ordinary attendant had administered an ounce of laudanum at a dose, without producing any effect on the system, which led Mr. Coindet to suppose that the remedies were decomposed in the stomach, and did not reach the circulation unchanged. He therefore, determined to inject a solution of opium into the veins. A scruple of opium was dissolved in distilled warm water and filtered. In the presence of several medical gentlemen, an opening was then made into the basilic vein of the right arm, and the point of a syringe introduced, when a drachm was injected every five minutes, taking care not to allow any air to pass in, and to move the point of the syringe as little as possible in the vessel. At the first injection the respiration became more regular, and less rapid—the state of the pulse and other symptoms remaining the same. At the second injection the breathing became quite natural—the pulse rose to one hundred, and grew fuller—the surface became elevated in temperature, and soon broke out in a gentle perspiration—the spasms were less violent, and the patient heaved some deep sighs like a person awakening from sleep. At the third injection the convulsions almost ceased, and she articulated some words distinctly. After the fourth and fifth injections, the patient entirely recovered. The operation was not followed by any alarming symptoms. The patient described the effect of the injections as if a torrent of liquid fire had been poured through the veins of the arm, concentrating in her chest, and thence dispersed over the whole body, attended with violent heat and pricking of the skin. These sensations were of a most painful nature. She finally became restored to health by the use of a bath, and a discontinuance of irritating purgatives, which she had been in the habit of using.

Case of Elongation of the Uvula producing Symptoms analogous to those of Chronic Pneumonia. By M. Cuynat, Chirurgien-major, &c.

A female, thirty years of age, born of healthy parents, and possessed herself of a healthy constitution, who had contracted a cough from frequent exposure to vicissitudes of temperature, after a year's suffering, presented the following symptoms, notwithstanding "the use of the most *heroic* and the most varied means which were considered proper for retarding the progress of a severe disorganization of the parenchyma of the lungs. The respiration was oppressed; a considerable constriction of the thorax existed; and acute, lancinating, and fugacious pains, increased on coughing and on full inspiration, were felt in that cavity; the patient was continually attempting to swallow or to hawk up mucus from the throat; a fixed pain, accompanied with tickling, existed in the larynx; the appetite was almost null; the tongue was

sometimes white, and sometimes in a natural condition; her pale countenance, extreme emaciation, depressed condition, both moral and physical, seemed, however, to indicate the profound lesion of some important organ; the chest, carefully explored, resounded very well in every part, the pulse was sometimes small, unequal, and at other times frequent and full, and frequently changed in less than half an hour to one or other of these conditions."

M. Cuynat, on examining the fauces, found the uvula elongated, loose on the base of the tongue, and distended with serous fluid. It occurred to him that all the symptoms experienced by the patient were occasioned by the elongation of this appendix. All internal treatment was discontinued, and the superabundant portion of the uvula was cut off. Fifteen days had scarcely elapsed, before all the symptoms previously indicated had ceased: the patient was soon completely restored to health.—*Revue Medicale*, Tom II. p. 353, and *Journal General de Medecine, Dec.* 1823.

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On Cancer of the Heart, with Cases. By MM. Andral and Bayle.—The development of scirrhous, and encephaloid or medullary structures in the heart, is doubtless very rare, as it has not been observed by many modern physicians who are much engaged in pathological research. MM. Bayle and Cayol say (*in the article Cancer, in the Dictionnaire des Sciences Medicales,*) that they have never seen cancer of this organ. M Laennec also says, that he has never observed in the heart any encephaloid structure, or any other species of cancerous disease. M. Recamier, however, has found the heart in part converted into a scirrhous substance resembling hog's-skin, in a subject which also had cancerous tumours in the lungs.

M. Cruveilheir (*Essay on Pathological Anatomy*) appears to have seen, in an old man, an organic lesion of the heart, having the characters of cancer. It consisted, he states, of cancerous masses of the tuberculous form, of which the greater number were seated on the surface of the heart, and a few only in its substance.

In 1813, M. Rullier read to the society of the Faculty of Medicine of Paris, a case of cancer of the heart (*Bulletins de la Faculte*, Tom. III. p. 357.) A man aged thirty-five years, of a rickety habit, and subject to catarrhs, was seized, soon after an attack of peripneumony, with a pain in the left side of the chest, with great oppression. The abdomen was painful, especially towards the right side, where he felt a considerable tumour. The patient always lay on his left side, which did not sound when struck. Œdema of the extremities gradually supervened, and at the end of much suffering he expired.

On *dissection*, the lungs were found in a healthy condition; but the heart was more voluminous than natural, and its cavities enlarged. The texture of this organ presented scarcely any traces of its primitive organization: the whole of its muscular sub-

stance formed not more than a twelfth of its mass: the rest, that is eleven-twelfths, was composed of a scirrhous, and partly carcinomatous substance, forming irregular prominences, of which the greater number extended from the external to the internal aspect of the organ. A large carcinomatous tumour was found in the mesentery: a scirrhous degeneration of structure was also observed in several parts of the parietes of the intestinal canal.

In a work recently published by M. Ollivier, on the Spinal Cord and its diseases (p. 262,) a case of cancer of the heart also is to be found. A woman, aged forty-six years, died of an attack of apoplexy at the Hospital Salpétriére, in 1822. No particular symptom had betrayed an affection of the heart before her death.

On *dissection*, the muscular fibres of the parietes of the right ventricle were observed to have been changed into a yellowish-white medullary tissue, offering all the characters of the encephaloid structure. This cancerous degeneration of texture occupied only the inferior half of the muscular parietes of this ventricle: the ventricle of the left side was sound.

Scirrhous tumours have been also mentioned, by Senac and Hildanus, as having been found by them in the heart. The three following cases place the occasional supervention of this form of organic lesion still farther beyond doubt, and are, in other respects, interesting.

Case 1st.—A man, aged fifty-six, was admitted into *la Charite*, in May, 1821, with all the symptoms of hypertrophy of the parietes of the left ventricle of the heart. His pulse was hard but regular. He complained of orthopnœa, and was anasarcous. He died on the eighth day after his entrance into the hospital.

"*Dissection.*—On bringing the heart into view, we were struck with the number of irregularities which its posterior aspect presented. In cutting through these irregular prominences, we found them formed of a very hard tissue, bluish gray and semi-transparent at some parts, at others white and more opaque, and marked by red lines which crossed each other, leaving between them areolæ of variable forms and sizes. This texture occupied nearly all the external paries of the right ventricle: the white colour appeared to extend into its internal membrane. In a few points only could the fasciculi of muscular fibres be discerned, and there they appeared discoloured and wasted. The muscular fibres were more distinct towards the internal surface of the ventricle. The columnæ carneæ were sound. The parietes of the right auricle presented throughout the same kind of morbid texture; and the part which separates the orifice of the two venæ cavæ was destitute of its fibrous structure. The cardiac artery was sound in the midst of the cancerous disease. The left ventricle was enlarged, and its aortic orifice narrowed by ossification."

Case 2d.—"A lapidary, thirty-seven years of age, had enjoyed a good state of health until his thirty-fifth year. He then became

asthmatic. During five or six months afterwards, he felt only difficulty of, and slight pain during, respiration: at the end of this period he was seized with a violent pain in the region of the heart, extending through the left side of the thorax. He suffered, at the same time, violent palpitations, increase of dyspnœa, vomiting, and a momentary loss of recollection. At the end of an hour the pain diminished, and, on the following day, he was restored to his usual state. During the year of his life that followed this attack, the pain on respiration increased slowly, and he experienced seven or eight paroxysms of the same kind as that now described: he compared the pain which he felt during these paroxysms to that of tooth-ache. He was admitted into *La Charite* about the end of November, 1821. At this period he was emaciated: his face was remarkably pale and yellow. He had an evening exacerbation of fever, without increase of perspiration. On examining the state of his organs, they all appeared regular in their functions; and we were at a loss how to explain the cause of the marasmus and hectic fever with which he was harassed. In the course of December he frequently felt lancinating and passing pains in the region of the heart: these were always of the same character. He also occasionally had palpitations; but in the intervals between these auscultation could not detect any very appreciable lesion of the heart. About the commencement of January the inferior extremities, then the superior, and, lastly, the face, became œdematous, and the patient died suddenly without pain.

"*Dissection.*—The muscles were pale and thin, and the body entirely deprived of fat. The external paries of the right ventricle of the heart was occupied by an irregular tumour, which, from its large size and form, resembled a supernumerary heart: it extended from the point of the ventricle to its base, and passed beyond this latter part, encroaching, to the extent of a few lines, upon the structure of the auricle. This tumour had replaced nearly all the muscular fibres, of which only a very thin layer was perceptible at the internal surface of the ventricle, and at several points it projected considerably into this cavity: it was entirely formed of the encephaloid tissue, partly softened. The other portions of the heart were sound, as were also the lungs. The cranial and abdominal organs presented nothing remarkable."

Case 3d.—"An old soldier, who had been in a state of mental imbecility for several years, had a tumour on his left cheek, which had rapidly increased, and, in July, 1821, had approached a state of ulceration. From that time to the first of October, an ichorous and fœtid matter continued to flow from his mouth; and the pulse was occasionally very quick. Respiration appeared perfectly free; the action of the heart excited no attention; and the patient preserved a considerable fullness of habit. He died the 23d of October, without evincing any new symptom deserving remark.

"*Dissection.*—The body was muscular and fat. The skin every

where presented a remarkable redness, resembling that of erysipelas, and differing essentially from the lividity frequently observed after death. The inguinal glands were swollen and red.

"The tumour of the left cheek filled the maxillary sinus, and rested beneath on the lower jaw: in this situation the gums were red and swollen. The superior maxillary bone and the teeth adhered to the tumour towards its upper and internal aspects, and were destroyed to a considerable extent. The part of the tumour projecting into the mouth was deeply ulcerated, and bathed in a brownish, ichorous, and very fœtid pus, which seemed to flow also from the carious bones. When divided in various directions, the tumour presented a uniform grayish colour, and a lardaceous consistence and appearance.

"About eight ounces of limpid serosity was found in the pericardium. The heart, which was about a third more voluminous than in its natural state, offered to the touch several of its parts considerably hardened. The cavity of the right auricle was much dilated, its parietes thickened, condensed, and possessing a grayish colour, and lardaceous structure. The orifice of the venæ cavæ was straightened. The circumference of the right auriculo-ventricular orifice presented no trace of its original structure, but was transformed into a lardaceous matter of an inch and a half thickness. The left auricle was altered in a similar manner, and the pulmonary veins were contracted at their orifice. The ventricles were sound, but the inter ventricular structure was in a cancerous state. The internal membrane of the auricles offered here and there lively red spots, arising from an injection of blood into the minute vessels. The mucous membrane of the intestines was red, and lined with a bloody mucosity."

"The facts which we have adduced are doubtless too few to enable us to trace the history and symptoms of this rare form of organic disease of the heart. A few corollaries, not entirely destitute of importance, may, however, be deduced from them. They evidently show that the muscular fibres of the heart can be partly destroyed, without any very evident derangement of the circulation. The third case shows that, in the heart, as in other organs, the cancerous structure produces no disturbance of the general nutrition of the body, as long as softening of it does not supervene. The second case does not contradict this position; for here there existed marasmus, hectic fever, and softening of the cancer. In neither of the cases was death the *immediate* result of the influence exercised by the cancerous disease on the circulation. In the cases recorded by MM. Rullier and Ollivier, the patients died of an intermediate disease. In the first of the cases here related, death seemed to result more directly from aneurism of the heart. Lastly, in the second case, it was owing to the change produced in the nutritive functions that the cancer of the heart occasioned the fatal termination."—*Revue Medicale, Fevrier*, 1824.

Dr. Copeland *on Cerebral Excitement and Blood-letting.*—We consider the following views of Dr. Copeland as of very great practical importance: on the younger part of the profession, in particular, they cannot be too strongly impressed, nor too often repeated. Cerebral excitement may be consequent upon inflammation, general plethora, or local determination; but it is not necessarily so, according to the prevalent but erroneous opinion. If we bleed a man, or other animal, frequently, either when in good health, or labouring under cerebral excitement, we shall produce, in proportion to the depletion, symptoms of determination to the brain—delirium—and death, with cerebral derangement up to the latest moment of existence; and, on dissection, the brain will be found gorged with blood, that is, in comparison with any other part of the body. The only exception to this is, where the loss of blood has been so great and sudden as to produce instant death. In profuse uterine hæmorrhage, how often is it necessary to watch the patient with the utmost care to prevent mental derangement? Irritative fever almost always supervenes in consequence of the cerebral excitement, and notwithstanding the uterine irritation, which might be supposed to counteract it.

Cerebral excitement and local determination are also produced most commonly—independent of depletion—by a low state, or debility of the system; and to those whose health is vigorous, the maxim "ubi irritatio ibi fluxus," seldom applies; and whether the irritation be physical or moral, the observed effects will be proportional to its intensity, and to the state of the system—*Anderson's Quarterly Journal, July*, 1824.

M. Beclard's *Extirpation of the Parotid Gland.*—This formidable operation, thought by several eminent surgeons to be impossible, was performed by M. Beclard, with what we may call partial success.

"L. F. Clout, a paper-stainer, was admitted into the Hospital La Pitié, on the 19th of August, 1823, on account of a scirrhous ulceration of the parotid gland. This man was aged forty-seven years, of an apparently good constitution, and of a sanguineo-nervous temperament. The disease had commenced eight years before; but, from being long indolent, and comparatively small, it had rapidly increased, and became the seat of lancinating pains: it had lost also its mobility. On admission into the hospital, this cancerous tumour possessed a very considerable elevation. At its superior margin it raised the lobe, and appeared to involve the cartilaginous portions of the auditory canal. It extended downwards more than an inch from the angle of the jaw; backwards, it adhered to the sterno-mastoid muscle, and its anterior portion covered a great part of the masseter. It was ulcerated in two situations, and had but little motion. There was no appearance of cancerous cachexia. The patient desired an operation, and M. Beclard performed it in the following manner:

"This patient having been placed on his side, the tumour was inclosed by two curved incisions, the one made at the anterior and posterior part, the other at the anterior and superior part of its circumference. That part of the tumour placed over the masseter was first dissected off, from before backwards, with sufficient ease. It was next attempted to detach it from below upwards; but a projection of its substance plunged deeply behind and beneath the internal pterigoid. To endeavour to remove this portion of the scirrhous at the commencement, with the rest of the tumour, would be to expose the operator to the risk of hæmorrhage, which might be difficult to repress during the operation; M. Beclard, therefore, decided to dissect upwards, by striking the bistoury into the structure of the tumour itself, on a level with the projections, whilst the instrument divided before and behind the cellular tissue, connecting it to the adjoining parts. Half the inferior circumference of the cartilage, contributing to form the auditory canal, which was enveloped in the diseased part of the parotid adjoining, was removed with it in this first dissection. Numerous arteries were tied at this stage of the operation; and M. Beclard continued the extirpation of the rest of the tumour. A portion of the anterior margin and internal surface of the sterno-mastoid, being converted into scirrhous substance, was excised with the bistoury. The projection situated behind the jaw was removed in successive slices: nearly the whole scirrhous mass was already dissected away in this manner, when a large jet of arterial blood announced the lesion of the external carotid, or the division of one of its principal branches. M. Beclard placed the fore-finger of his left hand on the place whence the hæmorrhage proceeded, seized the bleeding vessels with his forceps, and passed a needle with a double ligature around it; an assistant tied the vessel above and below the wound in it, which was lateral. The artery was held forward and outwards, whilst he completed the operation of that part of the tumour which remained in the pit, formed by the ramus of the jaw. One small projection of the tumour, placed before the cervical vertebræ, was only left. On account of its proximity to the internal jugular vein, M. Beclard contented himself with passing two ligatures beneath this part, by means of a needle, and with tying the one at its superior, the other at its inferior extremity.

"The considerable wound which remained after this operation, presented, in its anterior part, the masseter as if dissected, and the labial artery denuded, but not divided; at its posterior part, the mastoid apophysis, and the sterno-mastoid, with its interior and anterior portion removed; internally, the styloid apophysis, the external carotid surrounded with two ligatures, the stylo-hyoide muscle, and the digastric; and the small portion of the tumour which was tied, formed the bottom of the wound, which opened upwards into the meatus auditorius externus. The wound was dressed forthwith.

"The extirpated tumour presented scirrhous texture, mixed with a small portion of tuberculous matter. It was impossible to distinguish the structure proper to the gland itself.

"No particular occurrence took place during the first days of the treatment. All the right side of the face remained a stranger to any sort of expression, except what was occasioned by the action of the left side, producing some degree of motion in the soft parts of the right. The right eye remained continually open, which, owing to the irritation and dryness of the conjunctiva thus occasioned, gave rise to slight but obstinate opthalmia. At the period of suppuration of the wound, the patient experienced deafness of the same side, owing either to inflammation having been propagated to the internal membrane of the ear, or to the quantity of the discharge, and swelling of the adjoining parts. The suppuration was going on kindly, and healthy granulations covered the wound, when, on the twelfth day, the patient experienced rigours, followed by heat and fever. Phlegmonous erysipelas attacked the surrounding parts, and was followed with great delirium. General and local bleedings, during the three following days, caused but slight amendment. After this period the erysipelas on the face terminated by desquamation; but an abscess formed deep below the right trapezius muscle. The pulse remained frequent, and the derangement of the mental faculties continued. For several days the discharge was excessive. A counter-opening was made; and graduated compression was applied, which occasioned adhesion of the parietes of the abscess, the ligature placed on the external carotid, and that part of the tumour which was tied, came away. The wound was approaching to entire cicatrization, the fever subsided, and the strength and appetite returned; but to the preceding delirium succeeded taciturnity, which was only broken by paroxysms of rage; mental alienation had supervened to the febrile delirium.

"Three months after the operation, the wound was closed, unless near the ear, where it had assumed a character denoting a return of the cancer. The patient was still maniacal, and seemed affected with a chronic inflammation of the membranes of the brain. He died three months and three weeks after the operation.

"*Dissection.*—The external carotid artery was observed to terminate in cellular tissue, resulting from the cicatrization of the wound. There appeared no vestige of the parotid. The internal jugular vein was obliterated at the same height, and seemed to commence anew lower down, by communicating with the superficial branches. The corresponding lateral sinus was not obliterated. Some pus was found in the meatus auditorius externus. The membrane of the tympanum was sound. There was a marked injection of the pia mater, and of the choroid, which was redder than the choroid plexus. A serosity was found in the ventricles,

suspending particles similar to those deposited by some red wines."*

M. Weinhold's *Extirpation of the Parotid.*—This is another testimony in favour of this formidable operation. M. Weinhold says he has removed the parotid in three different cases with success. There are three stages, he says, of scirrhosity in the gland; the first that of induration; the second confirmed scirrhous; and the third open cancer.—In the first stage the gland may be extirpated by the scalpel; in the second, it is better to use ligatures and caustic; in the third, no remedy has been hitherto found. The great danger in the operation for extirpating, is wounding the carotid artery, or dividing the par vagum, either of which, would in all probability prove fatal.—*Anderson's Quarterly Journal for* 1824.

English Opium.—It is stated in the Transactions of the Society of Manufactures and Commerce, that the common poppy is cultivated very extensively for the manufacture of opium. Messrs. Cowley and Staines, of Winslow, produced one hundred and forty-three pounds of excellent opium in one season, from eleven acres and five poles of land, for which they received a premium from the society. A medal has been given by the same society, to J. W. Jeston, Esq., surgeon, for an improvement in collecting the juice of the poppy. This improvement consists in collecting the juice immediately after it exudes from the capsule, instead of allowing it to become dissipated on the capsule. The capsule is scarified with a sharp instrument, guaged to a proper depth, when the juice is scraped off with a kind of funnel-shaped scoop, fixed into the mouth of a phial. When one phial is filled the scoop is removed to another, and the juice is evaporated in shallow pans. Some varieties of the poppy are much more productive than others. The opium thus produced in England, has been used for several years by physicians and surgeons, who pronounce it superior to the best Turkey and East India opium.

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ART. I.—*An Essay on the Certainty of Medicine.* By P. I. G. CABANIS, Member of the Senate, and of the National Institute, Commander of the Legion of Honor, Professor in the School of Medicine of Paris, Member of the American Philosophical Society, &c. &c. Translated from the French by R. LA ROCHE, M. D. Philadelphia. R. Desilver. Octavo, pp. 119.

M. CABANIS has long been known as a zealous and ingenious vindicator of the Medical profession. Although it is not now so fashionable to ridicule the pretensions of those who profess a knowledge of human diseases and their appropriate remedies, as it was in the last century, when the love of hypothesis afforded frequent opportunity for the witty critic to display his humour, yet we are not entirely exempt from the persecution of a certain class of men, who disregard our principles because they are not mathematically demonstrable; and of another, who think that because we cannot in every instance avert the calamities which "flesh is heir to," consider us as fair game for their humorous propensities.

It is undoubtedly desirable that arguments against the usefulness of the medical profession should be answered, and that every enemy to useful science should be overcome.

In desultory conversation it frequently happens that ingenuity in argument is more successful than sound reason and confident knowledge, in determining a contested point. This arises, not from a deficiency of zeal and confidence in him who defends the truth, but from his ignorance of those arguments which are best calculated to disarm false logic of its imposing influence, and to expose the vanity of prejudice. Every man is conscious of the existence of some truths which he cannot demonstrate,—he believes, he knows, that they exist, yet he knows not how to convince another of their existence. Hence it is that men of learning, and men of great accomplishments in science and art, have so often become the subject of the satirist, or sat to the pencil of the humorist; and hence, also, is the amiable philosopher so often obliged to shrink from the coarse and mischievous railery of profane and unfeeling ignorance.

In recommending a work purporting to be a defence of the certainty of medical science, we do not seek an opportunity to prove the usefulness of physicians to the human family; we are aware that their claim to a rank among the best benefactors of mankind, is only opposed by those who are remarkable for their ignorance, their envious, their malicious disposition, or for unaccountable prejudice. But we wish to arm every member of the medical profession with weapons for the defence of his professional principles against every attack, from what source soever it may come; and although we are conscious of enjoying the respect of all reasonable men, we do not consider it an useless task to show them that their good opinion is not a mere compliment to us, but justly our due.

Medical science is an assemblage of various species of knowledge; or, we may say, of many natural sciences, which have no connexion but in the brain of the physician. Hence great variety of talent is necessary to the acquisition of a knowledge of medicine, and for condensing from a thousand sources its heterogeneous materials, and deducting therefrom general

principles. To form a system of medical practice which will include every case of disease, it is necessary that those sciences which contribute to medicine should themselves be perfect; this is not the case with any one of them; therefore, the physician, how splendid soever may be his talents, or indefatigable his industry, is not justly liable to censure for the exceptions which may be found to all the known laws of his profession, or to his most rational theories. But while we admit that much yet remains to be done in the vast field of medical science, we also claim credit for what has been done. We will not consent to be tried by the antiquated maxim,—"*Dato morbo, invenire remedium proportionatum.*" It is not pretended that the exact nature, situation, and extent of every disease is known, nor do we pretend to a knowledge in all cases of the most appropriate remedy for diseases which are best known. But this affords no argument against the certainty of medicine, because we are seldom confined to one remedy in any form of disease; one remedy may, on various accounts, be preferable to another, but because we are not able to discriminate justly between them, shall we reject the use of either? Again, we may not possess that accurate knowledge of the character and circumstances of a disease which would enable us, by the application of an appropriate remedy, immediately to arrest its progress,—yet there are phenomena which indicate its progress or its decline, thus testing the virtue of the remedies employed.

The phenomena which indicate the existence of disorder in the system, are very numerous and very dissimilar, as the particular diseases which give rise to them vary in nature, location, and extent. But a certain set of these phenomena are frequently observed to be coincident, and if the same phenomena are exhibited in the persons of several individuals, it is reasonable to suppose that the same disease exists in every case, and experience supports us in this opinion. By means of this kind we are enabled to discriminate between diseases, to study their characters, and to apply their remedies with a

very reasonable prospect of success,—notwithstanding that an Omniscient Being might have done the work in less time.

Physicians can appeal to the history of their profession for illustrations of the vanity, as well as the pernicious consequences, of that spirit of generalization which frequently arises from a desire to reclaim their profession from the charge of irregular empiricism. During the infancy of science and literature, when learning was confined to a few, it was important to every philosopher to have a system or doctrine of his own. A *system* ought to be the result of a careful observation of fact, but when fact is deficient, imagination supplies its place. Hence, the ancient history of medicine abounds in theories which display the poetic imaginations of their authors, rather than the investigating industry which characterises the modern benefactors of science. More recent observation has induced the cultivators of medical science to shake off the trammels of inconsistent theory, and to despise the pedantry of antiquity; and devoting themselves to the investigation of truth, they have accumulated a vast fund of knowledge, capable of being applied to useful practice. Thus, instead of disbelieving the evidence opposed to favourite theories—theories are now projected only on the authority of incontrovertible fact.

It appears, then, that the medical profession does not seek respect through the influence of an imposing theory, but demonstrates at every step its practical importance in affording an asylum to millions from the persecution of disease. Our author, in treating of the certainty of medicine, arranges the objections which are usually urged against the profession, under seven heads, by which means he is enabled to answer each by fair and distinct argument.

The first class of these objections are founded on the impossibility of investigating, *ad demonstrationem*, the principle of life. The second is drawn from the difficulty of discovering the proximate cause of disease. The third arises from the susceptibility of change, incident to some diseases,

and the complications of various disorders with each other Our ignorance of the *modus operandi* of medicines form a fourth class. The fifth consists of the uncertainty of the results of physiological and pathological experiments. The sixth regards the mutability of medical theories, and the frequency with which a new doctrine has been built on the ruins of a theory once considered as established. The seventh class of arguments against medical certainty, is founded on the difference of opinion which obtains among physicians themselves, and the consequent variety of practice that is observable among them.

Our limits do not admit of a detail of the ingenious and reasonable manner in which M. Cabanis has refuted the arguments of the enemies of medical art, on the above mentioned grounds. Nor, perhaps, would it be proper to give any more than an abstract of the most remarkable, as every intelligent physician who reflects on this subject, can draw from the stores of his own experience, a sufficient mass of evidence in support of all the positions which our author has taken.

It appears to be an unanswerable argument, that a profession, having for its object the protection of life against what are called morbific agents, must necessarily remain in a state of imperfection, and its practitioners must consequently be without confidence in their own prescriptions, while we are ignorant of the precise nature of Life,—whether it be a principle *sui generis*, an immediate cause of vital phenomena, or an effect of certain forms of matter, acting and re-acting upon each other, how matter becomes endued with life, and what circumstances are necessary to the connexion of this principle with matter. This is undoubtedly an interesting subject, to the investigation of which many have been prompted by curiosity, and also by the laudable desire of extending the boundaries of medical knowledge. But however important the knowledge of ultimate principles may appear to be, and however specious the arguments in favour of its necessity,

we must insist, that it would add but little to the utility of medical practice.

It is wisely ordained, that the various parts of the animal system are endued with a sensibility by which morbid action, or the presence of stimuli, capable of deranging the natural functions, are detected. Although we cannot demonstrate the cause of life, or the immediate cause of death, yet we are enabled, through the indication of a variety of symptoms, to discover the location of morbid action, and the nature of this action, sufficiently to guide us in the application of a remedy. Anatomy has discovered to us the structure of the human body, and shown us the organs by which its functions are accomplished, and also the connexion between various organs, by which pain and other indications of disease in one, is referred to actual disease in another. Hence a great deal has been done towards establishing a correct diagnosis, in which consists almost all the difficulties of the medical art. It is true, that our want of correct diagnosis, in a few instanees, affords foundation to a few *opprobria medicorum;* but how are these to be removed? not by searching for the essence of life, which, when known, would in all probability throw little or no light on the nature of the disease, nor, indeed, even assist in discovering its location.

Strict attention to the symptoms, the change which they undergo through the influence of certain medicines, with the age, habits, and constitution of the patient, and *post mortem* examinations, have done more towards the detection of error, and the improvement of medical science, than all the dreams of philosophy and vain reasoning upon ultimate and general principles. Let us not be charged with encouraging empiricism; we wish not to explode all systems and theories comprehending general principles of tried and acknowledged utility.—But we wish merely to insist that all doctrines, to be useful, must be drawn from the observance of fact. It is useless to erect a theory on any other foundation, nay, it is even in some instances pernicious, inasmuch as its erroneous im-

pressions oppose the establishment of truth, and it is more difficult to wean the mind from a favourite delusion, than to direct it successfully to the investigation of truth.

Many speculations have been entertained respecting the principle of vitality. The world has some time been amused with the contemplation of the vibration of atoms, and at another with an ethereal spirit. Sometimes life was a cause, having power over matter, and again, a mere effect of material combinations. But skill in these speculations is of no value at the bed-side of the patient,—vastly different is the path which will conduct the physician to a knowledge of remedies, and give him power over disease.

When we observe that certain symptoms occur at the same time, and that in every instance of the appearance of these particular symptoms, one and the same part has been affected with disease, and when we find also that different forms of disease in the same part, are invariably indicated by a corresponding difference of symptoms, we must acknowledge the agency of some general principle, which rules the animal economy by fixed and unchangeable laws. It is not necessary that we should be able to contemplate this principle in an insulated manner, free from its connexion with the phenomena of the organic system. Medical certainty only requires that such a principle should exist, and that the laws of the animal economy should be certain in their effects;—in other words, that the maxim, *similar causes under similar circumstances, will produce similar effects*, should be exemplified in the animated machine, as well as in the subjects of physical science. That this is really the case, has not only been long established by experience, but is daily demonstrated in the person of every living animal. These laws we discover in their effects, and we become acquainted with the existence of a certain disease in a certain organ, by observing a certain symptom or class of symptoms, which are known invariably to attend such disease.

Our knowledge of remedies is derived from the same

source, namely, observation and experience. We do not attempt to *demonstrate* that jalap will produce catharsis, tartrate of antimony emesis, or that certain applications will allay the violence and pain of inflammation,—nor could the most laborious and ingenious course of philosophising even lead us to such conclusions.—But what would we think of the intellects of a man who, on this account, would object to the use of these articles, when the certainty of their effects has been attested by invariable experience?

"Let us be judged by our professions," and it will then be found that the physician is not obnoxious to the charge of arrogance, though he may claim the possession of more knowledge than is usually acquired by other men. The medical profession is rich in practical rules, whose infallibility has been so established by experience, as to enable the physician to practise, not only with promptitude, but with certainty of success. If, then, experience is a sufficient foundation for practical rules, should the possibility of deriving rules from another source argue imperfection or uncertainty in medicine?

It may, finally, be argued, that there are some forms of disease that have hitherto baffled every attempt of the healing art. The greatest number, if not all such cases, when candidly considered, are harmless to the pretensions of medical science,—for they may be found to be in their natures incurable, to have arisen from original defects of the constitution of the patient, or to have, almost at the very commencement of their career, committed some irreparable violence to the system. Thus, we cannot charge to medical uncertainty the death of one whose lungs have been destroyed by ulceration; of another, whose arteries have become ossified; or of a third, who has suffered from the rupture of a cerebral artery; nor can we see how a knowledge of the vital principle could enable us to extend the aid of the materia medica to such cases.

With regard to the second objection, it is true that we have not so far gratified the refined taste of modern philosophy, as

to have traced every thing connected with the medical art to first causes.—But at the same time we insist that this is not a fair test of the merit of any art. We are all sensible of the advantages which we derive from the practice of various arts, the manipulations of these are said to be predicated upon certain principles;—if we examine these principles we find them to depend upon other principles too recondite to admit of demonstration;—thus, whatsoever path we take towards the discovery of a primary cause, we will find, that although reason may convince us of its existence, human ingenuity is incompetent to the task of its development. Let us grant that every art is conducted upon empirical principles, and that we make practical deductions from natural phenomena which are inexplicable, shall we therefore despise the benefits derived from such arts? Shall we refuse to eat bread because the baker cannot trace the phenomena of fermentation to a first cause? We know that the frequent introduction of digestible matter into the stomach is essential to the continuance of life, and we know that bread is digestible food, and we are ready to call him a madman who would object to its common use, by any train of argument founded upon our ignorance of the causes of its existence, the apparatus by which it is assimilated to the animal system, or of the laws by which the digestive apparatus is governed in effecting such assimilation.

So, also, in the art of medicine, we know, by long and unexceptionable experience, that certain substances may be necessarily applied to the human system, for the removal of pain and other symptoms indicative of disorder. Ample observation warrants the belief, that without the interference of such remedies, these symptoms of disorder would be the precursors of inevitable death. Is it then reasonable that we should refuse the remedy whose practical utility is established, because we may be ignorant of the abstract principle upon which it operates? Nor would a knowledge of the *modus operandi* of medicines render their operation more certain or satisfactory. We can avail ourselves of an effect of which we

know only the immediate cause, with as much certainty as if we had discovered the proximate cause to be the effect of one more remote.

The third objection is as easily refuted. A proximate cause of disease may, from time to time, vary in intensity, change its location in the system, involve in morbid action a lesser or a greater number of parts, exhibit features somewhat different under the various circumstances of constitution, habits, climate, &c. and perhaps become complicated with other causes. Nevertheless, there is always a certain train of symptoms sufficiently distinct to guide the attentive practitioner in his general prescription; and the cases are rare indeed in which, if from causes beyond his controul, he cannot avert the stroke of death, he cannot, at least, relieve his patient from much of the anguish of unrestrained disease.

It not unfrequently happens that this very complication of disorders, supposed to render the art of medicine too intricate for the scope of human ingenuity, points out a remote cause of the general disorder, and thus enables the physician, by striking at the source, to remove all the branches of diseased association. How often do we find the cough, threatening the horrors of consumption, the indigestion, nausea, and pain of a debilitated stomach, and the consequent head-ache disappearing before the medicine which has power over a disordered liver!

With regard to the fourth objection, it is of little or no importance for us to know the *modus operandi* of medicines. While we know that certain causes will produce certain desired effects, and that these causes are so completely at our command that we can at pleasure avail ourselves of their effects, and thereby accomplish our final object, surely it is mere curiosity that would prompt us to the investigation of the manner in which the effect is connected with the cause. If we wish to evacuate the alimentary tube, or to promote the action of the superficial exhalents, we know the substances whose exhibition will effect our object, and the result is just as satisfactory as

if we had followed them into the dark recesses of the animal machine, and witnessed their occult operations.

The fifth objection, we contend, is not well founded in fact. A great variety of experiments have been instituted for the purposes of ascertaining the nature and seat of various diseases, and of throwing some light on the *modus operandi* of medicines. It is true that many attempts of this kind for the development of truth, have not been followed by satisfactory results. Nevertheless, many of the labours of the experimentalist and the dissections of the pathologist have extended our knowledge of the laws of the animal economy, pointed out the peculiar features of certain diseases, and finally enabled us to prescribe with confidence, and save the lives of thousands. No single experiment, perhaps, is sufficient for the establishment of a law, however plausible its *rationale* may be and although it were admitted that no law, in any of the natural sciences, is perfectly established, yet there are many so rationally deduced from experience, that we are enabled to adopt them advantageously as rules of practice; many such rules, whose validity may not be absolutely demonstrable, have been applied to practice with such beneficial results as to place their utility beyond the reach of objections founded upon a few trivial exceptions.

By means of experiments the resources of the healing art have been much extended. All sciences and all arts are the result either of the accidental observance of natural phenomena, or of experiments instituted for the elucidation of preconceived ideas. Many of these have been ingeniously contrived and industriously conducted, with a view to certain results which have not been obtained. But although the desired object may not be attained, yet other discoveries are made, which prove to be of greater importance. Thus we trace the origin of the great science of chemistry to the vain attempts of the Alchymists for the transmutation of metals and the composition of an universal panacea.

But the labours of physiologists are not always unsuccessful. Not only has our knowledge of the nature of disease been much improved by them, but our *materia medica* has been greatly enlarged and corrected since it has become fashionable to pursue the experimental plan, and to seek causes through the contemplation of facts.

The next argument, according to our author's arrangement, against the certainty of medicine, viz. that "if the science of medicine rested on a solid foundation, its theory would at all times have been the same, and its practice more particularly would not have undergone, as it has, from one age to another, the most extraordinary changes," instead of exciting disrespectful feelings towards the science of medicine, ought to reflect the greatest honor upon the intelligence and industry of its cultivators.

Every science and every art has had an origin. With regard to each, a few observations gave rise to a few maxims, and constituted an imperfect system, susceptible of improvement, and perhaps liable to essential change, as in the progress of investigation, new facts came to light, requiring more comprehensive theories for their arrangement. Medicine, whether considered as a science or an art, has also had its birth and infancy. As a science, it is, and must always be, peculiarly susceptible of improvement. Its principles, as we have already remarked, rest upon the principles of a number of distinct sciences, none of which have yet arrived at perfection. Even mathematical science, one of the first that was cultivated, and one whose rules are more perfectly demonstrable than those of any other, is yet in a state of progressive improvement. But medicine is not a demonstrable science, it more particularly depends upon observation of insulated facts, and without being susceptible of being comprehended in any general theory, its practice is regulated by many maxims, independent of each other, yet resting on the best foundations, viz. fact and experience. Yet we may be asked why, if our science possesses some fixed principles, do

physicians practise differently in similar cases? This is undoubtedly the most popular as well as the most imposing objection that is urged against our scientific pretensions.—But it is easily answered.

In the first place, with regard to the difference which obtains between the practice of the ancient and the modern physicians, it is sufficient to mention the progressive improvement that medical science has undergone; in the course of which, new facts have overturned old theories, and increasing knowledge in this, as well as in other sciences, has removed deep-rooted prejudices. New doctrines and more enlarged ideas, are the natural result of successful investigation of fact, therefore the novelties of modern practice are a natural result, and as such are exempt from animadversion. Secondly, that cotemporary physicians should differ in many particulars in their treatment of certain diseases, may arise from the unimportant nature of the matter in question. But their differences frequently involve a general question, apparently of great importance, insomuch that two physicians will treat the same disease according to different systems of practice. It is true that such is often the case, but in such instances the disease admits of various methods of treatment, and each method may employ a great variety of medicinal substances to the same purpose. There are but few, very few, diseases that are conquerable only by one particular remedy;—the generality of them are vulnerable at many points, they will yield to any one of a variety of remedies. Hence the difference which the unlearned think that they detect among physicians, is so far from casting disgrace on the profession, that if all physicians should treat a given disease in one and the same manner, it might be regarded as one of the most remarkable of fortuitous coincidences. Many of the differences between cotemporary practitioners also arise from the circumstance of the continual improvement of medical science. The old practitioner, who has long since lost the enthusiasm which stimulates the student to industrious research and to

an intimacy with the latest improvements in each branch of his profession, applies in practice those principles which he learned in his youth, and he differs from a practitioner of more modern date, either from his ignorance of recent discoveries, or from prejudice in favour of early impressions. Hence the folly of ascribing wisdom to age, and skill to decrepitude.

But if individuals are to be found who do not perfectly understand, or properly apply the principles of their profession, still this is no argument against the profession itself, either as regards its utility, or the stability of its principles.*

Before we dismiss this part of the subject, we must consider another and a very fruitful source of animadversion upon our profession. It has been remarked above, that diseases were liable to be complicated with each other; or, in other

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* With regard to this part of the subject, it may not be improper, nor perhaps, altogether useless to remark, that the medical profession is frequently charged with the faults of those who are in no way connected with it. In every part of the world ignorant pretenders to knowledge in the healing art, may be found, and there are but too many credulous people willing to lend them their support. The consequence is inevitable.—Disappointed in the relief which they expected to derive from medical art, the dupes of quackery vent their anathemas against a science which they never employed, instead of ascribing their misfortunes to an undue faith in newspaper advertisements and the use of nostrums, having no claim upon their confidence.

It is too frequently the case, particularly in the southern and western sections of the United States, that the character of our great and noble profession is in the guardianship of ignorance and prejudice. The empiric who boasts of certain secret remedies, unknown in the universities, or too valuable for gratuitous publicity, will frequently acquire both fame and fortune, notwithstanding his innumerable and often, fatal errors. The failures of such, and of others not less remarkable for an impudent substitution of a certificate of membership of a medical society, for a diploma, ought not to be brought in account against the certainty or the utility of medicine.—Yet to the shame of the community, that submits to such imposition, these form the most fruitful source from whence the lovers of detraction draw their vexatious arguments against professional merit.

words, that various causes of morbid action may exert their influence upon the human system at the same time.* Hence various shades of disease arise, which indicate as great a variety in the application of remedy. These things can be seen by the experienced and judicious practitioner, and he compounds his medicines accordingly. But in every case he is expected to give a name to the disease; he does so for the gratification of his patient, well knowing the uselessness of medical nosology, and aware of the imperfections to which it must always be liable. Yet if he does not prescribe for this *name* as another practitioner would do, in a case so dissimilar as to call for dissimilar practice, a disgraceful reflection is cast upon the profession by those whose ignorance renders them totally incapable of forming the least degree of judgment in the case.

M. Cabanis has placed many of the cavils against his profession in their proper light; and by exposing the folly in which they originated, he has shielded "the great science" from the profane attacks of its unprincipled enemies; and some enemies it will probably always have, for the successful cultivators of this science are justly considered as a superior order of spirits, whose acquirements are beyond the reach of the common order of men, and it is but natural, that perfection, if it exists on earth, should be sought for here. But, alas! so little are men disposed to be satisfied, that leaving respectable mediocrity out of the question, the most extravagant poets can invent nothing sufficiently devilish for the depravity of their taste, nor, on the other hand, can the most finished models of human virtue, talent, and industry, satisfy their unreasonable craving after ideal perfection.

Much credit is due to Dr. La Roche for his correct trans-

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* We have chosen this form of expression for the sake of brevity;—we include in the term morbific agents, unfavourable states of the atmosphere, improper food, pernicious habits, and every thing that can affect the health of the system.

lation of this excellent work, by means of which it may be much more extensively useful in this country, than it could be in its original language. H.

ART. II.—*An Essay Concerning Tussis Culvulsiva, or Whooping-cough, with Observations on the Diseases of Children.* By BENJAMIN WATERHOUSE, M. D. Boston, 1822.

WHATEVER merits we may feel disposed to recognise in the present essay, it will certainly appear evident to all those who have already perused its contents, that the credit of offering a methodical arrangement, and clear exposition of the various subjects of which it is composed, cannot with propriety be conceded to its author. In order, as he avows, to "enliven a dry subject," Dr. Waterhouse has thought fit to incorporate among his observations on the whooping-cough, long and dry anatomical descriptions, and very many physiological and pathological disquisitions, as applicable "to asthma, dyspepsia, or consumption," and he might have added to many other diseases, as to the one forming the main object of his inquiries, and which, although they may have served admirably well, the purpose of swelling the volume to the respectable size of one hundred and fifty-two pages, are nevertheless, in our opinion, altogether useless, and calculated only to render the reading of it fatiguing and painful, and a review of its contents still more so.

Dr. Waterhouse sets out with the declaration, that he was led to commit his thoughts of whooping-cough to paper, from observing the difference in opinion among physicians of the nature, causes, and treatment of this painful distemper. From this we are naturally led to conclude that his intention is to throw, if possible, some light on this obscure subject, an intention which recommends him to our warmest praises; but whether or not he has succeeded in the attempt, and whether we shall know more on this disease *after*, than we did *before*,

reading the present volume, are questions we shall next endeavour to determine.

In respect to the history of whooping-cough, our author is inclined to believe, that it was first described in the seventeenth century, and says, that the anxious student could see no traces of it through the dark ages, "that he could learn nothing of it from the books of the Arabians, and that even after the resurrection of letters in Italy, down to the art of printing, he could not discover that the distemper had ever existed." "It is strange," he continues, "terrible and destructive as this disorder is, that we have no description of it in any book prior to Dr. Thomas Wallis, who taught at the university of Oxford, and practised in London between the years 1650 and 1680; of course, cotemporary, several years, with Sydenham." In a review of this nature, it is not, perhaps, necessary to dwell with much minuteness on the precise moment when a disease has been first described; nor is it our intention to do so. Nevertheless, we cannot refrain from remarking, *en passant*, that our author, in confidently refusing the credit of first describing Pertussis to Wallis, has laboured under a small mistake. In all attempts to establish with precision the date of particular events, the strictest attention should be observed,—and the minutest investigation should not be neglected. Had Dr. Waterhouse been attentive to this wholesome precept, and extended his researches into works of an older date than those he has consulted; or had he ever opened Sprengl's History of Medicine, he would without difficulty have found that Mezeray, in his History of France, describes the disease as having prevailed epidemically in France, in the years 1414 and 1510;—that Coyttarus and Pasquier mention it as having appeared again in 1557, and that it again made considerable ravages in Europe in 1580. But we cannot too strongly insist on the little necessity of examining when and where a disease, such as the whooping-cough, has first made its appearance, and been noticed by medical writers. Consistently with this opinion, therefore,

we hold the conjectures of Nil de Rosenstein, respecting its African or East India origin, or his endeavours, agreeably to the fashion of his time, to discover some passages that have an obscure reference to it in the writings of Hippocrates, to be of no value to science or humanity. A study of books will assuredly teach us when, and by whom, it was first described, but never when it for the first time appeared;—since all the causes of the disease having in all probability existed from the creation of the world, the malady itself, though not described, must necessarily have afflicted its earliest inhabitants. Such indeed *must* be our opinion, unless we adopt the belief of Dr. Husson, who, in his work on vaccination, advocates the hypothesis of the identity of whooping-cough with the distemper of dogs. If this opinion be correct, and if it be established that the distemper of dogs is not the product of the same causes as whooping-cough, then we might perhaps believe that, in 1414, by some accident or other, the disease was communicated, for the first time, from a dog to the human species.—We marvel that Dr. W. has not thought of this hypothesis.

But having dwelt sufficiently long on Dr. Waterhouse's Prolegomina, we proceed to notice the body of the work; the first chapter of which is appropriated to an enumeration of the many names which at various periods, and by different authors, have been employed to denote the whooping-cough—to their various etymologies; and also to a lengthened description of the malady. "Dr. Robert Watt, of the city of Glasgow," Dr. Waterhouse says, "who has written industriously on this distemper, tells us that in North Britain, it is conspicuously called whooping-cough, and *kink-cough*, and hence the derivation chin-cough. This Scotch provincialism, kink, is, it seems, synonymous with fit, or paroxysm; for they say in Scotland, in common parlance, "a *kink* of laughing,"—"a *kink* of crying,"—and "a *kink* of coughing." The phrase is a novelty in our country. Dr. Johnson suspects that the term *chin* cough may have come from

the Dutch word *kinckin*, to pant, or breathe short,—yet we doubt the whole of it. "Now chin-cough is a corruption of the Dutch, or rather Teutonic word kindhoest, and means simply child's-cough, from *kind*, a child, and *hoest*, cough; and is precisely synonymous with the Latin term *Tussis Puerilis*. It was necessary to say thus much, because many people have an idea that *chin* cough, is a milder species of whooping-cough."

In respect to the symptomatology of whooping-cough, Dr. W. not content with presenting an original and distressingly minute description of the various forms assumed by the disease, has introduced Dr. Wallis's, both in Latin and in English. What the advantages are which may arise from this accumulation of description, other than the one we already hinted at, we are at a loss to determine. The detail of symptoms, which he has himself given, was sufficiently long and precise, to supercede the necessity of adding a very imperfect one, extracted from a work familiar to many. Be this, however, as it may, we feel little disposed to transcribe here the six or seven pages which the author has devoted to this part of his subject, as whooping-cough is already well known to our readers—and because, were this not the case, faithful descriptions of it may be found by a reference to any of the elementary books of medicine, and more especially to Dr. Dreyssig's excellent work on medical diagnostics.

On the subject of the different stages of the disease, Dr. W. observes, "it is customary of late, with most medical writers, to divide every acute disease into three distinct stages. We are not certain that we can draw the line with due precision in the tussis convulsiva. It appears to us, however, that the characteristic symptom, the *whoop*, is formed between the fifteenth and twentieth day; and that a change or crisis appears about the fortieth; but when it continues longer than the fiftieth, it becomes chronic; and then it may run on to six months and more, with occasional remissions. During this indisposition, the weakest organ or function suffers; or

that disorder to which the constitution is most liable, aggravates the original malady." He further remarks, that he should have remained ignorant of some particular symptoms of the whooping-cough, had he not recently suffered under it, and compared his symptoms with those of other adult patients, qualified for relating their sensations correctly. From them he also learned that, during the early paroxysm, the urinary organs, from the kidneys downward, felt unusually sensitive; while the stomach had now and then a glow, accompanied with a transient nausea, occasional *horripilatio*, and some other signs of a disordered system tending to fever.

The succeeding chapter of the work before us, contains an inquiry into the seat of the whooping-cough, together with observations on the mucous membranes. On the seat and nature of this affection, much diversity of opinion has prevailed among medical writers. Whilst by Willis, Hoffman, Fothergil and Cullen, the disease was referred purely to a spasm; by others, among whom was Sydenham, it is regarded as a disease of high inflammatory character; and, on the contrary, by Brown, as one of asthenic action, requiring the Sthenic treatment. Some physicians have placed its seat in the stomach. Among those who advance this opinion, Pinel stands conspicuous. Referring the primary link of the disease to that viscus, he regards the irritation of the lungs as the effect of a purely sympathetic action; nevertheless, by one of those inconsistencies with which his work, in common with all other nosological arrangements, abounds, he classes whooping-cough among the *neuroses* of the respiratory organs. Buttu considers the disease to depend on a deranged state of the primæ viæ.

"It is not very surprising," says Dr. Waterhouse, "that physicians should differ in opinion as to the nature, and the cause of whooping-cough; but it must appear strange to the ordinary reader, that medical men are not yet agreed as to the *seat* of it." In order, therefore, to clear up, in some measure, the almost impenetrable obscurity in which the subject

is involved, he sets out by asking, under what head in nosology, we are to locate this disease? And, in the next place, endeavours to decide this *important* question. We fear, however, we shall have little reason to congratulate him on his success; or be satisfied with our own,—in placing before our readers a clear summary of his opinions, owing to the great confusion existing in the selection, as well as in the arrangement of the materials composing this chapter. He commences his subject by endeavouring to prove that *post-mortem* examinations can throw but little light on the nature of this and other diseases; and that consequently Dr. Watt in asserting that, having had recourse to dissections, he had derived from these the most solid satisfaction, since they pointed out the nature of the whooping-cough at once, and settled every doubt in his mind respecting the mode of treatment which ought to be pursued, had advanced more than could be satisfactorily proved. "I value *necrotomy*, he says, but have less confidence in *post mortem* appearances, than in years past. All we can see in the dead body is redness, swelling, ulceration, and fracture. Besides, seeing, as well as hearing, and pulse feeling, have their grades of perfection, depending on cultivation."—"The cold, stiff, dead, collapsed, and half putrified cadaver is so different, so widely different, from the warm, turgid, panting, pulsating, breathing, and all moving, living body, that it requires no small experience, nay, it requires often consummate judgment to determine accurately the cause of symptoms, and of death. We are ready to grant that the nature, and even the seat of many diseases, have not, hitherto, been determined satisfactorily by *post-mortem* examinations; and agree with our author, that hydrophobia, some cases of tetanic affections, of epilepsy, and, indeed, of the whole class of spasmodic disorders, leave so few traces behind them, that we cannot derive from the minutest dissection a very perfect knowledge of their nature. But this does not do away the utility of dissection, or refute the correctness of the observations made by Watt and others, respect-

ing the seat of whooping-cough. But let us see to what results Dr. Waterhouse arrives on this latter subject. "If," says he, "we cannot fix precisely on the muscular part which is primarily affected in this distemper, there can be no doubt as to the *membranaceous* part. We are certain that its seat is in that delicate and very irritable *internal skin*, denominated the *mucous membrane*, which lines all the passages from the eyes, nose, ears, mouth and throat, downwards, including all the emunctories, outlets, and inlets of the body." After this declaration, our author indulges in anatomical and physiological researches on the mucous tissue, into which, however, we shall not follow him; and he next endeavours to ascertain the portion of the membrane primarily affected in whooping-cough, and as his notions on this particular subject, if not original, are at least explained in a manner peculiar to himself, we shall transcribe them in his own words. "It is presumed," he says, "that on this mucous membrane thus constituted, endowed and governed by the general laws of life, impressed on the whole system, *the specific contagion of whooping-cough first seizes and operates.* It does not appear that it affects the whole mucous membrane of the head and breathing organs at once, as in catarrh; but it seems at first to occupy a small part only, and thence to creep on by degrees. The parts first attacked, appear to be extra-thoracic; and we have reason for believing that they are the portion of the mucous membrane, which covers the upper parts of the alimentary canal, including the pharynx, œsophagus, and that entrance of the stomach called the cardia; when the mucous membrane is observed to be disposed into longitudinal folds, conformable to its power of dilatation. It thence extends to the diaphragm one way, and to the larynx, trachea, and air-vessels of the lungs, in extreme cases, another; but in most instances, the lungs are affected by consent or sympathy; and not organically, until the distemper has changed into a chronic disease. Even then we can hardly allow it to be bronchitis, whose origin is catarrh, but a state or condition of the respi-

ratory apparatus, that wants a name; and is more like the chronic state of measles, than any other."

Passing over Dr. Waterhouse's long remarks, and the whole train of reasoning by which he endeavours to prove that there exists an essential difference between whooping-cough and catarrh in croup, we proceed to notice his third chapter, in which he resumes the investigation of the mucous membranes, and adds a disquisition on the diaphragm. "From what has been said," he observes, "it appears highly probable that whooping-cough has not the same cause, nor does it, in the first instance, occupy the same parts of the mucous membrane, with *catarrh*, *croup*, or *bronchitis*. It appears, also, that the *Systema Spiritale Pneumonicum*, is not primarily, but secondarily, or sympathetically affected in whooping-cough.

"Where then is the primary seat of this terrible distemper? The more I contemplate the human frame, while in health, and the more I scrutinize the same body disordered, the more I am confirmed in the opinion, that if to the *stomach*, and *duodenum*, is added the *diaphragm*, we shall find that *they are the original or primary seat of nearly all the internal distempers that flesh is heir to.*" It appears pretty evident, from this exposition of our author's theory, respecting the seat of whooping-cough, that he has not offered any thing original; since it has already been shown that Pinel, among many others, placed the primary link of the disease in the stomach, and regarded the affection of the respiratory organs as sympathetic. In many respects, the reasoning of Dr. W. is erroneous, and he frequently involves himself in assertions that are at variance with correct anatomy, and the true principles of physiology. His notions concerning the extension of the disease from the diaphragm, will serve to prove this. In many parts of his work he admits, in this disease, the existence of spasm, which he well knows exists only in the muscular fibre; but here, neglecting to refer the affection of the diaphragm to a spasmodic condition of its fibres, he says, that the disease is seated in the mucous mem-

brane of the stomach, and duodenum, and "thence extends to the diaphragm, and to the larynx, trachea, and air-vessels of the lungs," from which one unacquainted with anatomy would naturally conclude that the diaphragm is lined with a *mucous* membrane.

For our part, the more we see of whooping-cough, and reflect on its nature, the more we are disposed to coincide with Darwin, Watt, and the disciples of the physiological school of France, in regarding it as consisting of an irritation or inflammation of the mucous membrane of the respiratory apparatus, extending itself, sometimes to the parenchymatous substance of the lungs. Nor do we hesitate to state it as our decided opinion, that in many respects it differs from bronchitis only in the spasmodic affection of the muscular fibres of the diaphragm and other parts, which supervenes after a few days, and sometimes weeks continuance of the disease, and that the vomiting and other gastric and intestinal derangements are the effects only of the spasm already noticed, or of accidental complications, which are likely to occur in every affection of the chest. In refuting the opinion of Pinel on this subject, Broussais remarks, "we cannot doubt that a gastric irritation is capable of exciting cough; examples of it are daily met with; but it is only a special *exaltation* of the sensibility of the mucous tissue of the lungs that can determine the convulsive cough of Pertussis. This *exaltation* itself depends on a mode of inflammation, equally peculiar, either in its cause or degree, of the mucous lining of the trachea, or bronchæ; but this species of catarrh passes very easily beyond the limits of the irritation producing the cough, in order to change itself into a very intense inflammation of the pulmonary apparatus, and to complicate itself with gastro-enteritis." We are well aware that a modification of this theory of irritation, has been proposed, and has received the approbation of some names of no little respectability in the profession; but we must confess that it does appear to receive support from the established principles of physiological

pathology. It is maintained, for example, that the irritation which constitutes the disease is purely nervous, and bears no analogy to common irritation. This difference between the nervous and vascular irritations we have never been able to discover, since the former invariably precedes the latter for a longer or shorter time, and the latter runs into the former;—this is well exemplified in the disease which forms the subject of our present inquiries; for, in every instance which is denominated the *nervous* irritation of whooping-cough, the disease assumes all the characteristic features of a true vascular irritation;—in other words, of a violent inflammation. That an analogy exists between this disease and bronchitis, is evident from the circumstance that, like the latter affection, the whooping-cough appears under the same modification of atmosphere, reigns epidemically together with it, cannot be distinguished from it in the early stages, even by the most experienced practitioners, and resembles catarrh; and when chronic, terminates in phthisis, with or without tubercles.

Postponing our inquiries into the contagious character of the whooping-cough, we return to Dr. Waterhouse. He seems to lay a considerable stress on the immense influence of the phrenic or precordial regions over the rest of the economy, and indulges in many remarks with the view of proving, that long experience, particular experiments, and our own feelings, demonstrate that there is no part of our system so liable or sensible to impressions. On the subject of this phrenic centre, in which Van Helmont had located his great Archeus, and which Buffon, and many others, regarded as the seat of the soul, our author has repeated, though in a language far less perspicuous, the ideas which the celebrated Borden and Lacaze had long ago offered in the "Idea de l'Homme Physique et Moral," and in the "Specimen ex novo Medicinæ Conspectu." After much discourse on this important centre, which he qualifies with ingenious titles of "Head quarters of the animal economy;"—"The focus of our best, as well as our worst feelings," he remarks, that the structure of the dia-

phragm would render it probable that it is an organ destined to more important offices than that of a mere partition; or *floor for the heart* to lie on, or the *leather of the bellows*, to follow mechanically the impetus of the air, or the living actions of the lungs and *ribs.*" And after a very minute anatomical description of this organ, and of its nerves, he concludes that it is *very probable that the centre of this organ, where the aorta and vena cava pass from above downwards, and from beneath upwards*, is the seat. source, receptacle, or *citadel* of that nervous energy which is suitable to the organic life and action of all the abdominal apparatus, and on which they depend for support;" consequently the seat of the phrenic region.

The succeeding chapter, on epidemics, is probably one among those the author has introduced, for the laudable purpose of "enlivening a dry subject," and we should certainly have stopped to notice some of the many remarks he has made on the subject, had they appeared to us to have possessed as much of that enlivening virtue as he would seem to attribute to them. We shall not, therefore, draw the attention of our readers from the subject more particularly under consideration, for the purpose of amusing them with speculations relative to the coincidence of comets, and volcanic eruptions, with pestilence, meteors in the atmosphere, with sickly seasons; the sickness and death of quadrupeds, fish, and insects, with wide wasting epidemics. "We have not been able," he says, "to trace the whooping-cough to any local cause, nor to any known peculiarity in the seasons. It appears in all places, in every season; and in all sorts of "*constitutions*" of the year; in the open country, as well as in the confined city. It does not appear to seize more readily any particular constitution, or habit of body."

From the foregoing observation it would seem evident, that Dr. Waterhouse entirely rejects atmospherical agency in the production of whooping-cough, and that, faithful to an opinion

he has already advanced, he regards the disease as arising solely from specific contagion.

But if it be denied by some, that the whooping-cough is a disease produced by the same general causes, as catarrh, pleurisies, peripneumonies, and is void of contagion, it must be conceded, that in some years, and in some seasons, it prevails more exsensively than at others, and even assumes an epidemical character, diffusing itself over a large portion of country; —attacking a great number of individuals, of all ages and conditions, and proving, at some times, more fatal than at others. In 1580, this malady proved fatal to 9000 children in Rome; and, according to Nil de Rosenstein, it was fatal in Sweden to 43392 children, from the year 1749 to 1764 inclusively, or 2712 in each year. Dr. Waterhouse himself (p. 17—18) has afforded us proofs, that the disease occasionally assumes the characteristic features of an epidemic; for, borrowing from Watt's treatise, he tells us that, in the fifteen years prior to 1717, only 63 were registered in London as having died of chin-cough, and 56 of cough, and that in the fifteen years ending with 1776, they amounted in the same city to 4252;— differences that could depend on no other cause than a difference in the atmospherical constitution of these different periods. Dr. Waterhouse, in reasoning on the passages we have just quoted, makes the following question: "Are we to infer from these facts, that the malady is in itself absolutely incurable, or that it is always a slight distemper?" And next answers, "neither one nor the other. We ought, by no means to conclude, that every disorder which we cannot arrest and hinder from running on a tedious length, is in nature unconquerable." From this latter remark we had naturally concluded that Dr. W., like Huffeland, Werlhof, and many other ontologists, entertained the belief that pertussis must run its course, and cannot be arrested in its progress; when, on turning to the very last page of his essay, we found the following passage, which exonerates him, if not from the charge of inconsistency, at least from that of a belief in that erroneous

doctrine. "When we are distempered and the body acting wrong, we may arrest the particular disorder by depriving it of the power of doing mischief in that particular way, by means of copious and repeated bleeding. It (the whooping-cough) may be arrested in a few days by repeated emetics and purgatives, by mercury, alone or combined with antimony."

Without enlarging more on this subject, we proceed to lay before our readers another passage from the work under review, in order to show the author's opinion of the nature of the whooping-cough. In conformity to the opinion he entertained respecting the nature and origin of all diseases, Linnæus taught, that the present malady was the product of a distinct species of insects, which, being inhaled, found a retreat in the bronchial vessels. Now, as eruptive diseases have been referred by some pathologists, even of the present day, to the same cause, Dr. Waterhouse's notion will not be found much at variance with those of Linnæus. We shall quote his own language. "If we know nothing of the remote cause of whooping-cough," he says, "we know but little more of its nature. Experience teaches us that it attacks a person but once in life. From every view we are inclined to believe it an inflammation *sui generis*, and strongly suspect that it is of the exanthematous class; occupying, at first, a small portion of the membrane." Professor Cullen, after saying that whooping-cough seems to proceed from a contagion of a specific nature and singular quality, adds, that it does not, like most other contagions, necessarily produce a fever; nor does it, like most others, occasion any eruptions, or produce otherwise any evident change in the state of the human fluids. From our own observation and feelings, we are led to think, that there is always more or less fever, and not unfrequently eruption also. In small pox and measles we can discover eruptions in the roof of the mouth and fauces, and those eruptions probably extend farther than we can see. They may, like apthæ, sometimes extend throughout the

whole of the alimentary canal." This view of the pathology of whooping-cough, although presenting an appearance of ingenuity, is, we believe, on the whole, incorrect, since dissections of those who have died of the disease, have revealed no such appearances, (at least in a very large majority of instances) as apthæ, or indeed any other species of eruption on the mucous tissue of the stomach and intestines. That the mucous glandulæ, abounding so plentifully in those membranes that are the seat of the disease, may be, and sometimes are, considerably enlarged, is a fact that cannot be denied; but this circumstance is widely different from the existence of any specific eruption, and depending merely on the encreased excitement, or to speak the language of the day, on the irritation of these parts.

We pass by our author's observations on the "Breathing and Vocal organ." In this part of his essay he gives an anatomical description of these organs,—an account of the mechanism of the voice,—a physiological explanation of the different species of coughs, and a disquisition on dentition,—the whole intended to show, "that irritation and morbid affections of parts below the thorax, even as low down as the bladder and hemorrhoidal veins, produce a convulsive action in the breathing organs, expressed by a cough."

The next chapter is devoted to the subject of "Dissections, and the appearances in diseased bodies after life." Its insertion here is, we believe, improper, as occasioning much repetition that might have been avoided.

He mentions, that in some dissections recorded by Dr. Watt, there were found long patches of inflammation on the small intestines, and on the external parts of the stomach, looking as if painted with coffee grounds; and a universal crust of lymphatic exudation on the internal surface, with here and there streaks of red;—these appearances, he admits, evince a pre-existing state of inflammation. Now it appears from the dissections of the same Dr. Watt, that in a great majority of instances, the lungs were discovered to be the seat of violent

inflammation, or its consequences, effusion, adhesion, suppuration, &c. Consequently, if Dr. Waterhouse feels disposed to deny the evidence presented by these dissections, of the existence of disorganization, or of the lungs, we cannot understand how he can venture to maintain, that some dissectors have shown redness, &c. in the intestines, and that these prove a pre-existing inflammation in these parts, the evidence should be deceptive or satisfactory in both cases.

Dr. Waterhouse next enters on the *nature of whooping-cough, deduced from the operation of certain remedies,* and endeavours to adduce, satisfactorily, evidence of the location of the disease in the alimentary canal, and not, as is believed by many, in the pulmonic organ. "Have not all those medicines most efficacious in mitigating the severity of whooping-cough, a special reference to the alimentary canal, rather than to the trachea, bronchia, or pneumatical system of the lungs?" In this belief of the possibility of discovering the seat of whooping-cough from the operation of remedies found beneficial in it, Dr. Waterhouse has merely repeated the opinion of Pinel, who, after referring the seat of this disease to the stomach, says, "From this arises the utility of evacuants, antispasmodics, and tonics, in subduing the extreme sensibility of the digestive organs. We deem it almost superfluous to point out the fallacy of this belief, and the erroneousness of the assertion, that a disease is of gastric origin, because evacuants are found of advantage in its cure. We need only remark, that on the same principle, croup should be regarded as a violent disease of the stomach, because emetics and cathartics are used in it with the most decided benefit.

Among the remedies employed in the treatment of the whooping-cough, our author first notices emetics, which he thinks always give temporary relief. In this opinion we entirely concur. Notwithstanding their proscription by those who asserted they were fatal, emetics must be regarded as remedies of great efficacy in pertussis; and in this light they have been considered by all intelligent writers on the subject. Not to

mention names of an older date, we observe that Burton employed them whenever the primæ viæ were loaded with phlegm, by which he thought the absorption of remedies in the lacteals was prevented; and by Buttu they were judiciously regarded as the natural cure of the disease. But emetics do not act beneficially in whooping-cough simply as evacuants, but also by making a strong impression on the system, by determining to the surface, and producing a counter irritation in the stomach, by which they contribute to mitigate the violence of the inflammation, or even shorten its duration. In order therefore to derive from them their full effect, they should be repeated often, avoiding, however, their exhibition whenever the stomach is found to be in a state of irritation, as is frequently the case in this malady. But although emetics generally are of decided advantage, some choice exists in respect to the particular article to be employed. By many, the emetic tartar is preferred, whilst by others, ipecacuanha has been regarded as possessing a more decided efficacy. Both of these medicines are good, and may be used to meet different indications. Thus, whenever it becomes necessary to evacuate the stomach of its foul contents, and this organ gives signs of being too highly irritated to admit of the exhibition of powerful remedies, the ipecacuanha may be used with the greatest advantage, "because," as Dr. Pierson has judiciously remarked, (Prize Essay, p. 13,) "no quantity which can be easily swallowed at once, has ever been known to produce a deleterious effect,—a charge to which the preparations of antimony are all liable when given in full doses." On the other hand, antimonials will be found more serviceable, whenever the necessity of a strong and revulsive action will be fairly indicated. Tartar emetic, in small doses, combined with some testaceous powders, and an occasional dose of magnesia at night, were the principle remedies recommended by Fothergil; and he derived from them the most decided benefit in the cure of the whooping-cough. We recommend the following remarks in respect to the time of administration, as well

worthy of attention. "This time of day (say between breakfast and dinner,) has been fixed upon preferably to any other, on these considerations, viz:

If the puke is given early, the stomach being empty, a straining too great to be borne without detriment might be occasioned. Was it given in the evening, the child would, perhaps, be robbed of too great a quantity of nutriment."

Some physicians have recommended nauseating doses, and cases occur in which this practice may, perhaps, be sometimes beneficial. Frederick Clossius recommended highly the golden sulphuret of antimony, in quantities so small as not to be productive of any action on the stomach; and in France, according to Bosquillon, the ipecacuanha and Kermes' mineral were prescribed in a similar manner. But he very wisely remarks, that these medicines do good only when they excite vomiting; and in general we think it will be found that nauseating medicines distress children too greatly to admit of their general exhibition.

Next to emetics we shall speak of fumigations. Of the many substances employed for that purpose, tar is the most common, and found peculiarly beneficial in relieving, by the vomiting it excites. Dr. Watt speaks in terms of praise of this remedy, and other writers have corroborated his statement of its benefits. The tar may be employed, either simply, by filling the room with its fumes, procured by placing a dish filled with it in hot coals, or else in the manner recommended by Dr. Wansborough of London. The method of this gentleman, which is far preferable to any other, consists in placing some tar in a vessel having a tube for the passage of the fumes. A heated poker is next introduced into the tar, and the fumes issuing through the tube are made to be respired by the patient. In respect to the *modus operandi* of tar fumigation, we cannot agree with Dr. Waterhouse, who, forgetting that the remedy has been employed with success by Crichton and others, in genuine affections of the lungs, remarks, that the tar fumigation proves that the seat of the disorder may be any

where rather than in the mucous membrane of the trachea and bronchiæ. The reverse is, we believe, the truth; nor can we acquiesce in the assertion that the larynx is at all times a sentinel that instantly repels such an invasion. The fact that these fumigations are beneficial in relieving oppression arising evidently from wrong action in the lungs, proves that this sentinel is not so vigilant as our author would seem to think, and does not so invariably repel the invasion. Not content with this, our author adds, although in so doing he evidently contradicts himself, that this repulsion takes place more especially in this disease, when the larynx is *rendered more sensitive by a peculiar inflammation.* In all cases of *whooping-cough there is an increased sensibility in that portion of the mucous membranes covering the larynx, and pharynx,* which repels acrid fumigations, and *creates trouble in swallowing dry substances.*"

The next medicines Dr. W. mentions as affording relief in this disease, are purgatives. And because the venerable Heberden tells us, that "in disorders of the lungs, emetics frequently procure easy breathing, but that cathartics are so very seldom useful, as scarcely to deserve to be even tried;"—he naturally concludes, "that experience, in opposition to theory, places the primary seat of the distemper in the alimentary canal." Dr. Waterhouse seems to be an advocate for mild purgatives in the treatment of whooping-cough. "Late writers," he says, "recommended strong, and often repeated cathartics; but neither sound practice, nor legitimate theory, can justify the procedure. Parents, and also young practitioners, have recourse, rather too often, to drastic purgatives in the ordinary complaints of children. This practice destroys the healthy balance between the nutritive and absorbent systems; and if it does not lay the foundation of scrophula, it certainly developes a scrophulous habit in some who would otherwise have passed through life without such appearances." These remarks on the dangers attending a too violent system

of purgatives in the diseases of children in general, and in whooping-cough particularly, must be regarded as judicious; for although a discharge from the alimentary canal proves beneficial in a great number of their diseases, still the frequent and sometimes daily administration of violent purgatives cannot but prove dangerous in many instances, by producing an undue and too long continued irritation in the mucous tissue on which they are applied, and in this way laying a foundation for numerous and fatal maladies. Of the purgatives employed in this disease, we have generally derived most benefit from the calcined magnesia, or castor oil, either administered alone, or preceded by small doses of the submuriate of mercury. This latter substance should never be overlooked when the alimentary canal is disordered.

By Sydenham, bleeding was earnestly recommended in the whooping-cough. "By this practice," he said, "of venesection, and repeated purgatives, and by this alone, is conquered the convulsive or whooping-cough, and obstinate disorders which scarcely any other method will subdue." We believe that few physicians will be found to refuse their testimony in favour of the occasional advantage derived from blood-letting, in the treatment of this disease.

Cases are frequently met with in which symptoms of excitement run so high as to necessitate the prompt and free use of the lancet; but in a majority of instances emetics, cathartics, and the remedies we shall mention in the progress of our review, will be sufficient to arrest or mitigate the disease, and to do away the necessity of blood-letting. "We have seldom resorted to the lancet in whooping-cough," says Dr. Waterhouse, "unless some accessary symptom, in plethoric subjects, has urged it; as to obviate a threatened apoplexy, or where there has been some great and sudden exposure to cold and wet, inducing pleuritic inflammation. This is, however, an accidental disorder, superinduced on the original regular distemper."

Leeches or cups, we have known to be very serviceable in this disease. Faithful to the views he has adopted, in respect to the seat of the whooping-cough, Mr. Webster (Lon. Med. and Phys. Journal) recommends leeches to be applied to the forehead, and behind the ears, "for there the blood being drawn from parts supplied by the ramifications of the supraorbital arteries, and of those of the internal ear, it is drawn more directly from the head than it would be were the leeches applied to the temple." Upon the same principles, he thinks, the jugular vein may be opened with advantage; though he has not had recourse to this operation in any case.

We have no doubt, that applied to these parts, leeches will occasionally prove of great service in the cure of the whooping-cough; but we would limit their employment in this way, to those cases in which symptoms of irritation or congestion of the head are manifested—in those instances in which blood, during the paroxysm, rushes to the head in too great quantity, as well as in those attended with convulsions. But such symptoms are not invariably found; and in the generality of instances, leeches and cups to the *chest* will be more serviceable in subduing or mitigating the inflammation of the mucous membrane of the bronchiæ and trachea, or parenchyma of the lungs.

In this stage of the disease, (first) blisters and other external stimuli have been had recourse to, and from them the most decided benefit has been derived. In the generality of instances, indeed, blisters will do away the necessity of bleeding, and may be applied early in the disease. "Under the impression," says Dr. W, "of whooping-cough being a simple inflammatory affection, *blisters* have been applied after bleeding and purging. They were used by Willis, and approved by Cullen. Those who conceived it to be a spasmodic disorder, had recourse to blisters and to embrocations, in which camphor, turpentine, oil of amber, and garlic, have predominated. These, by exciting a counter irritation, have,

now and then, afforded temporary relief, and, with change of air, effected cures. Few things have been more extolled in Europe, than a solution of tartar emetic, mixed with a strong tincture of cantharides, and used by embrocation; which it seems gives relief in proportion to the violence or rage of the eruption produced by it on the skin."

In the second stage of the disease, when by the remedies we have already mentioned, all symptoms of high inflammation have been removed, cantharides have been administered internally, and received high eulogiums for their beneficial effects. So early as the year 1735, the practice was introduced by Burton, who used them in combination with bark and camphor, in a mixture containing a small quantity of the balsam copaiba. By Sutcliff, the remedy was employed in the liquid form, and in this way it is better suited for children.—Lettsom used it also with success, as well as Millar and Hillary. Whether administered in the solid or liquid form, a certain portion is given three or four times a-day, until a slight stranguary is excited; and this is considered the criterion of its efficacy.

Dr. Waterhouse, although entertaining the opinion that the internal, as well as external, use of cantharides, is sometimes productive of great inconvenience and distress in causing stranguary, especially in hot and *very dry* weather, says, nevertheless, that there can be little doubt as to the efficacy of cantharides, and balsam capivi, in relieving, and sometimes curing whooping-cough, "and its œtiology," he continues, is not very difficult. "They both stimulate, if not vesicate, principally, the membranes of the urinary organs, from the kidneys to the urethra. The effect is most felt in the neck of the bladder, and also, probably, in that irritable point in the internal surface of the bladder, mentioned by that eminent anatomist, Charles Bell, directly over the junction of the muscles of the ureters, with the internal sphincters of the bladder; the point which, when inflamed, provokes an in-

cessant call to make water. If, after the constant use of these powerful medicines, we could see the mucous membrane of those patients, I have little doubt, but we should find them efflorescent throughout, and in proportion to this efflorescence, would probably be the diminution of that pathognomonic one in the œsophagus, diaphragm, larynx, and upper orifice of the stomach."

Remedies included in the class of antispasmodics, have all at various times been employed in the cure of the whooping-cough. Whether or not their efficacy may be accounted for on the principle of a new, strong, and ungrateful impression on the membranes, rendered highly sensitive by disease, as asserted by our author, we shall not pretend to decide. True it is, however, that these remedies, and more particularly the assafœtida, are of great benefit in removing or relieving the spasmodic cough that remains after the subsidence of the more inflammatory symptoms, and constitute, after the employment of the antiphlogistic remedies, the principal means employed by the most enlightened and experienced physicians of this country.

Castor was introduced in practice with much commendation, by Dr. Morris (Lon. Med. Obs. and Inquiries, Vol. III. 281.) In combination with bark it produced, according to his report, considerable alleviation of the whooping and retching attendant on this disease. But subsequent trials, by other practitioners, do not seem to have confirmed the accuracy of his report, regarding its beneficial effects. The musk has also been recommended, and was used by Hillary for the purpose of relieving the convulsions which sometimes supervene in this disease. The artificial musk was also introduced; but these medicines, like the castor, have lost the little and ephemeral reputation they had at one time acquired.

In this state of whooping-cough, tonics, both of the vegetable and mineral kind, have been employed by different practitioners, and in many instances with the most decided

advantage. Among the former, the Peruvian bark stands conspicuous. By Burton it was said to be as successful with him in this disease, as in the cure of the ague. On the continent of Europe it was used with success, and received the praises of Rosenstein, Whytt, Bisset, and many others. Speaking of its virtues, Dr. Cullen says, "I consider the use of this medicine as the most certain means of curing the disease in its second stage; and when there has been little fever present, and a sufficient quantity of the bark has been given, it has seldom failed of soon putting an end to the disease."

The nitrate of silver has likewise been employed, with what success we hardly know;—the remedy having been made little use of in this country. Its violence would seem a sufficient reason for discarding it from practice among children. Ferriar and Simmonds used the arsenical solution with advantage even in cases of young children.

By Dr. Forbes, of Edinburgh, the internal use of lead has been recommended. He employed the liquor sub-acetatis, or Goulard's extract. Notwithstanding, however, the praises he bestows on this medicine, we would caution our young readers against its too *bold* administration, owing to its giving rise, in many cases, to painful and sometimes dangerous affections of the bowels. The same remedy seems to have been used in France for a long time past, in combination with lischen pixidatus. (See Le Medecin de la Campagne, Vol. II. p. 290.)

The various narcotics have been extensively tried in the second stage of this disease. Although having the testimony of Hufeland and of Demachy in its favour, opium has, in general, been found of not much utility in this disease. Dr. Willan, however, whose authority is of considerable weight in medicine, found the watery solution of this substance very serviceable in mitigating the paroxysms, and prolonging their intervals. We have often had recourse to the various preparations of opium, and have never derived from their use much decided

advantage. Digitalis has been regarded as much superior to opium, and consequently prescribed pretty extensively in this disease. Of this article Dr. Pierson* speaks favourably. "Six drops," he says, "of a saturated tincture, may safely be given to a child one year old, at intervals of six hours. This dose may be increased by one drop at a time, till a diuretic effect is produced, or the state of the pulse indicates its diminution or suspension." Hemlock has also been used in this disease; but notwithstanding the high encomiums bestowed upon it by Dr. Butter, it has been found of not much value. By many, Belladona has been used. Shoeffer, Hufeland, and Wetzler, in Germany, and Alibert and Janin, in France, speak favourably of its virtues.

Dr. Granville, of England, as well as some physicians of this country, bestow praise on the use of the prussic acid; whilst, on the contrary, Dr. Elliotson, of Europe, and Dr. Pierson, of Salem, have not found it to display much power over this distressing complaint. We have given it a trial in several instances, and have never derived from it any decided benefit.

Some popular remedies have been sometimes found of advantage in the whooping-cough. Among the negroes of the West Indies, castor oil and molasses mixed together, and administered often in the day, are said to afford effectual relief from the distressing symptoms of this complaint. "Equal portions of linseed oil and flower of sulphur," says Dr. Waterhouse, "have been used with remarkable success in mitigating the violence of whooping-cough. Some people consider it the sovereign remedy in this distressing complaint. A tea-spoonful of this mixture, taken three or four times a-day, or less frequently, according to the age of the subject, has certainly produced beneficial effects."

Change of air has always been regarded as peculiarly beneficial to patients suffering under the whooping-cough. In-

* Prize Essay,

stances are not wanting to prove that children transported many miles from home, have insensibly lost their convulsive cough, and from a state of complete languor, and too sick to take nourishment, have revived after a day's journey, and sucked with relish.

"But breathing another air," Dr. Waterhouse remarks, "comprehends not all the advantages resulting from a journey. Transport a man fifty miles, in a close carriage, while asleep, and unconscious of his removal, merely to breathe an air hanging over another soil, he probably would gain but little advantage from it. It would hardly alter the morbid sameness of thought and feeling. But let his migration be through a varied country, diversified with hills, vallies, rivulets, plains, and forests; so journeying in places different from the one he long inhabited; resting in houses unlike his own; meeting people differing in manners and customs from his; partaking food differently cooked; and drinking water from a different soil; these things, added to the exercise, insensibly operate a change in mind and body, and generate healthy feelings in the adult valetudinarian, especially if he be hypochondriacal; and they have more or less effect on children; for pleasing novelty is a healthy stimulus."

"It appears, that those who are suffering in the country, under asthma, or whooping-cough, gain the same benefit from removal into the city, as the inhabitants of a large town, similarly affected, reap from removing out of it. We have observed that children go from one country village to another, twenty or thirty miles distance from the salt water, with the same favourable result. They go from the Highlands to the low fishing towns, and from the margin of the broad Atlantic to the mountains, with equal good effect. In these cases, it is in vain to talk of the good air, or the bad one, of certain places. It is the change; it is the new impression that overcomes the morbid one."

Dr. Waterhouse next treats of the "*Analogy between Whooping-cough and Dysentery.*—"I have long enter-

tained the opinion," he says, "that if whooping-cough and dysentery were not very near akin, they were in that degree of relationship, as to bear traits of a family likeness. Both infest the mucous membrane; both affect, especially, the alimentary canal as one *great gland*, beginning at the pharynx, and ending at the rectum. Both are accompanied with violent convulsive affections of the diaphragm; both are derived from contagion; and both, we suspect, are exanthematous affections of the inverted skin, or mucous membrane of the œsophagus, stomach, and intestines."

After this declaration, the author notices the cases recorded by Dr. Watt, and observes, that whenever in these the whooping-cough had been violent, or near the fatal close, the discharges from the bowels were very dark and fœtid, with an appearance of small fragments of membranes diffused through them—that in most cases, where the bowels had been very lax, involuntary discharges, great irritation, and a prolapsus ani have accompanied the paroxysm. Again he remarks that "they could never pronounce the patient out of danger, until they had procured something like the natural fæces. "Here," he exclaims, "the resemblance between the dysentery, and disease under discussion, is striking. Would it be straining the analogy to say, that what tussis convulsiva is in the one, tenesmus is in the other. By which we mean only, so nearly resembling, as to refer them both to the same law of the system, whenever the mucous membrane of the alimentary tract is principally affected."

We are indeed at a loss to ascertain what Dr. Waterhouse pretends to have discovered. It seems to us, that all he has proved in the foregoing observations, amounts to this fact, and no more, that whooping-cough is the effect of the irritation or inflammation of one portion of the mucous membrane, and dysentery of the inflammation of another portion;—that in cases of the former disease, dysenteric symptoms occasionally occur; or in other words, that the irritation, constituting

the whooping-cough, extends to, or attacks at the same time, the living membrane of the intestinal tube, and complicating itself with a spasmodic action of the muscular fibres of that canal, gives rise to symptoms of the dysentery. Than this, nothing can be easier to conceive; and to prove it did not necessitate so much labour, since all physicians, familiar with the laws of irritation, are perfectly aware of the phenomena of its extension. In our opinion, therefore, the same analogy exists between whooping-cough and dysentery, as between catarrh and the same disease; both are the effects of inflammation seated in the mucous membrane, but modified by the nature of the part affected.

In respect to his favourite hypothesis of internal exanthematæ, and his opinion that dysentery is a distemper belonging to that class, we shall say little, since he has not adduced one single remark or fact calculated to support the assertion. His quotations from Aretœus, Bonetus, Dolœus, Cleghorn, Fraser, Donald Monro, Sir George Baker, and Linnæus, who he poetically denominates the "Celebrated Secretary of Nature," amount to the fact, that in dysentery the intestines are highly inflamed, and are found, on dissection, to contain a greater or less number of ulcers, or ulcerated tubercles; and not at all that there existed a specific exanthematic disease. Were ulcers signs of a disease of the latter class, then chronic catarrh, consumption, typhus, and other kinds of fever, diarrhœa, and, indeed, almost all diseases attacking the mucous tissues, and which, in the true spirit of a zealous nosologist, he undoubtedly regards as totally distinct in essences from dysentery, would depend on the same cause, or probably on a different species of eruptive disease. We hope Dr. Waterhouse will not venture to maintain such an opinion, and we are really sorry he has asserted as much as he has done on the subject of these ulcers; since all his arguments and reasoning have only tended to prove satisfactorily to us, that he has neglected to avail himself of the labours of our modern

transatlantic brethren, and of some of our own countrymen, and thus failed to keep pace with the improvement of knowledge.

The next and last portion of this work to which we shall direct the attention of our readers, is his chapter *on the power of vaccination in removing or mitigating Whooping-cough.* On this subject we are of opinion, that the observations of Dr. Waterhouse are entitled to considerable attention. His indefatigable researches on vaccination, are sufficiently known; and we are not a little pleased to learn the results of his extensive experience on the power of small-pox and kine-pox in removing or mitigating pertussis.

On this subject a great diversity of sentiment is discovered in the writings of those who have made the whooping-cough and vaccination the objects of their investigations. By some, the fact of small-pox, and the vaccine disease, coexisting in some instances with the whooping-cough, has been regarded as a strong argument against the opinion of a preventive power in vaccination against pertussis; and Dr. Pierson, in his essay on this latter disease, states, that he has never, in his numerous experiments, witnessed the fortunate results attributed to vaccination. Mr. Bertrand, on the contrary, arrives at a very different conclusion from his experiments; and Valentine, in his work on vaccination, and in his biographical notice of Jenner, lends support to the opinion of Bertrand. He remarks, that he witnessed many cases in Nancy; and says of Jenner, that he had noticed several times, that if we vaccinate after the first twelve or fifteen days of the whooping-cough, the disease is mitigated, and is finally cured more promptly. Husson, in his "*Recherches sur la Vaccine,*" quotes from the report of the Commission of Milan, the case of a girl who, during eighteen months, was afflicted with a convulsive cough, which disappeared as soon as the inflammation of the vaccine had gone through its different stages.

He likewise mentions, that many children, affected with whooping-cough, were vaccinated at Brussels, and experienced relief proportioned to the development of the vaccine disease. "I was once sanguine on this head," says Dr. Waterhouse, "and believed almost all I wished; so that, when my children were suffering under whooping-cough, I regretted they could not be revaccinated. Subsequent experiments made me less sanguine, but have not influenced me to relinquish entirely the opinion, that the variolous, as well as vaccine fever, diminishes greatly, and sometimes entirely obliterates the symptoms of the epidemic convulsive cough.

"I have never had repeated positive proof that vaccination arrested and subdued the distemper in its first stage; that is, before the eighteenth or twentieth day. In some few cases I thought it prevented the absolute formation of the disease, when there was reason for believing that the subject had been infected by the contagious cough. But the sagacious physician will here see the chances for deception.

"I feel more confident, that after the distemper had reached its acmé, and was on the decline, that then vaccination, when pretty strongly marked with febrile symptoms, has operated an alteration for the better. That small-pox would modify measles, is not a new idea; and that it has modified whooping-cough I have no doubt. Since vaccination has chased away small-pox, some think that measles and whooping-cough have increased in frequency, and malignity.

"I am of opinion, from observations, that children vaccinated in the spring, or early in summer, are not so liable to cholera in the autumn of the same year. In this, I believe, I am not deceived; yet its confirmation, or rejection, is left to the experience of others." R. L. R.

ART. III.—*Characteristic der Franzoesischen Medicin, mit Vergleichenden hinblicken auf die Englische.* Von JOH. LUDW. CASPER, M. D. &c. &c. Leipzig, F. A. Brockhaus. 1822, Octavo, pp. 608.

Characteristic of French Medicine, &c. &c.—It requires no very extensive acquaintance with medical literature to perceive, that medicine, both in relation to its theory and practice, exhibits national features as characteristic as the physiognomy and manners of nations themselves. In no country, perhaps, do we find this fact more strikingly illustrated than in France. The Parisian school, especially, exhibits the most obvious peculiarity in this respect. It possesses, as our author very correctly observes, an experimental, matter-of-fact character, in which we perceive very little of the spirit of philosophical generalization, or of refined speculation upon the nature of phenomena. The French have carried into medicine the philosophy of *Condillac.* The senses mark out to the the limits of inquiry. The dissecting knife and the crucible, conduct them to the end of their researches. They rarely carry the "mind's eye," beyond the phenomena which these disclose to their senses; and the fields of recondite inquiry,—of deep abstract speculation are but seldom entered. It is from this cause that, with the exception of the writings of Broussais perhaps, we look in vain for any such thing as a system of *general pathology* in French medicine. The French are indefatigable observers of phenomena; they are continually enlarging the sphere of our knowledge by experimental researches and minute examinations of the *sensible* conditions of the animal system; they are industrious in augmenting the mass of materials, whilst but very few attempt to work up these materials under the plastic powers of the mind into a regular and connected edifice of science. With the Germans the thing is quite different. With them the philosophy of *Kant* is the

mens quæ agitat molem. Whilst they are the most industrious people under the sun, in collecting and recording facts and observations, they are also the most addicted to that refined and sublimated speculation which characterises the Kantean school. They consider it not enough to be acquainted with phenomena and their obvious relations;—they look for higher views than those which the senses afford;—they endeavour to find out the hidden relations of phenomena,—to penetrate into their nature, and to draw from them general and fixed principles. Hence their medical literature abounds so much with works on *general pathology*.

In relation to these two distinct traits of the German and French schools of medicine, that of the English holds a middle rank. Its character is neither so speculative as German medicine, nor so experimental as that of the French. The English devote less attention to the mere observation of phenomena than the French, and waste less time in speculating on them than the Germans. Their logic is more searching than that of the former, and less vague and wandering than that of the latter. The *inductive* mode of philosophizing is more closely pursued by them than by either of the other two nations. Without, however, pursuing this topic any further, we pass on to a review of the work before us, which, in a style at once pure, elegant, and perspicuous, presents a very full and striking view of the characteristic traits of French medicine.

In the second chapter, Dr. Casper gives a circumstantial account of *the internal police of the French medical schools, and of the course and plan of instruction prescribed by government.*—The old system of the French universities, which had existed, with very little variation, for more than seven hundred years, was in 1820 dissolved, and a new one raised in its place, which, in its whole organization, shows the influence of a jealous power, whose entire policy is dictated by a spirit anxious for the perpetuation of those

principles which are to fix the stability of thrones, and arrest the progress of just and liberal sentiments. That our readers may have an idea of the severe discipline and *surviellance* to which the students of the French universities are subjected under the present regulations, we give the substance of the principal ordinances in relation to this subject.

The *Commission* of public instruction have ordained:

1. That in order to obtain the degree of doctorate, the student must obtain a *certificate of inscription* from the faculty which he attends. This certificate must be exhibited by the student when he presents himself at the lectures of the faculty.

2. Admission may be gained to the public lectures without being regularly inscribed, by means of an *admission card*, obtained from the faculty. For this purpose a register, signed by the dean and secretary, is kept, into which those who wish to obtain an admission card must enter their names, age, place of birth, and actual residence. If they do not reside in Paris, they must exhibit a certificate of permission to remain in this city. Admission cards to the lectures can be denied to no one who has complied with these regulations.

3. Every one who attends the lectures of a faculty, must, when requested by the dean or the professors, exhibit his *inscription* certificate, or his card of admission. This is noted on the card or certificate, which is then again returned.

4. If any disturbance is created by a person holding an admission card, the card may be annulled at the request of a professor and the dean, by a resolution of the faculty. If any one is detected in lending his admission card to another, the person so detected, loses his card. If an admission card is annulled, the possessor shall be requested by a written note, to return it to the secretary within three days. If he neglect doing so, his name, with the number of his card, shall be put up in the halls of the faculty, and the janitor shall deny him entrance if he presents himself for admission. The person

who has thus lost his card, may apply for a new one; the faculty determine whether he shall have another card or not.

No expense is incurred either for a regular inscription, or for a card of admission. The cards must be renewed at the beginning of every new course of lectures.

By the king's decree, issued in July, 1820, it is ordained that:

1. After the 1st of January, 1821, no one shall be admitted to the first inscription in any medical faculty, who has not previously obtained the degree of *Bachelor of belles-lettres.* (Bachelier—és lettres.)

2. From the 1st of January, 1823, no one shall be admitted to inscription in any medical faculty, who has not previously obtained the degree of *Bachelor of Arts.* (B.—és Sciences.)

3. From the 1st of November, 1823, every one who applies for a *first* inscription in any medical faculty, or secondary school, must exhibit:—1. A certificate of birth;—2. If he is a minor, a certificate attesting the consent of his parents or guardian to his attending medical lectures; this certificate must designate the place of residence of the parents or guardian. —3. Finally, the diploma of the requisite bachelor's degree.

4. From the 1st of November, 1823, no one shall be admitted to inscription in a faculty or school, which is located in any other town than that in which the parents or the guardian resides; unless he be presented by a person who lives within the district of the faculty or school; and which person is then obliged to enter his name and place of residence in a register kept for that purpose. The student has then his domicil with this person, and to which all notifications relating to him must be sent. Should this person, who stands in the place of the parent or guardian die, or go on a journey, the student is bound to obtain another in his stead, otherwise every inscription which he may have obtained since the death or absence of his sponsor shall be annulled.

5. The student, also, must give in his actual place of residence;—and whenever he changes this, he must notify the faculty of the change. Every false notice, or neglect of notice, may be punished by annulling his inscription.

6. Every professor in a medical faculty, or secondary school, is bound *to call out the names of those students who attend his lectures at least twice every month.* If the number of students is too great to be called out at one time, the professor may call out the names of a part of them daily, so that all may be called up at least twice in each month. No notice is to be given of the day on which the names are to be called out. If any one is detected in answering for another, he shall lose his inscription. Every student who has been twice absent in a quarter of a year, without a proper excuse, shall have no *attest of his industry* from the professor.

7. Inscription certificates are only given for the three months in which the student has obtained *attests of industry* for *all* the lectures which he is attending.

8. Every want of respect, every act of insubordination in a student, towards a professor, shall be punished with the loss of one or two inscriptions. In case of a repetition of the delinquency, the student may be excluded the lectures for a term not less than six months, nor more than two years. The same punishment shall be inflicted on those who excite the other students to disturbances within the schools.

Every student who shall excite, or be the cause of disturbances *without* the walls of the schools, or who may take part in any public disturbances, shall be punished with the loss of not less than two, nor more than four, inscriptions, or be excluded from the lectures, for a period of not less than six months, nor more than two years. The student may appeal to the commission of public instruction. A repetition of the offence, is punishable by an exclusion from *all* the schools for a term extending from six months to two years. This exclu-

sion from all the academies is dictated by the commission of public instruction. The student may appeal to the council of state.

The students shall form no associations of any kind whatever, without having obtained the consent of the local authorities, and without having previously informed the rector of their intentions and object. They are also prohibited from acting and writing as a collective body.

The following course of studies is prescribed for those who desire to obtain the degree of Doctor of Medicine.

First year. First, Second, Third, and Fourth Inscription.	Winter.	Anatomy Physiology Chemistry.
	Summer.	Hygeiene External Pathology Botany.
Second year. Fifth, Sixth, Seventh, and Eighth Inscription.	Winter.	Anatomy Physiology Operative Surgery.
	Summer.	Hygeiene Pharmacy External Pathology Chirurgical Clinic.
Third year. Ninth, Tenth, Eleventh, and Twelfth Inscription.	Winter.	Operative Surgery Chirurgical Clinic.
	Summer.	Internal Pathology Materia Medica Medical Clinic.
Fourth year. Thirteenth, Fourteenth, Fifteenth, and Sixteenth Inscription.	Winter.	Medical Clinic History of Medicine.
	Summer.	Internal Pathology Forensic Medicine Clinique de Perfectionnement Midwifery.

The following is a list of the Professors and Officers belonging to the established Medical Schools of Paris.

Leroux, Perpetual Dean, and Professor of Clinical Medicine.
Boyer, Chirurgical Clinic.
Chaussier, Anatomy and Physiology.
Deyeux, Medical and Pharmaceutical Chemistry.
Dubois, Clinique de Perfectionnement.
Hallé, Hygeiene.
Lallement, Operative Surgery.
Pelletan, Examinator, Internal Pathology.
Richard, Examinator, Botany and Natural History.
Thillaye, Keeper of the Museum.
Desgenettes, Adjunct to Halle.
Dumeril, Secretary of the Faculty, Anatomy and Physiology.

De Jussieu, Botany.
Richerand, External Pathology.
Vauquelin, Chemistry.
Desormeaux, Midwifery.
Dupuytren, Chirurgical Clinic.
Moreau, Librarian, Medical Literature.
Royer-Collard, *Examinator*.
Béclard, Adjunct to Dubois.
Marjolin, Surgery.
Orfila, Toxicology.
Fouquier, Internal Clinic.
Roux, Surgical Clinic.

Besides these professors whose lectures the student is obliged to attend, there are other lecturers of great eminence in Paris, attached to other institutions. Thus in the *Jardin des Plants*, Cuvier, Lacepede, Blainville, Decandolle, and others. In the *College de France*, Thénard, Portal, Biot, and others, and in the *College du Plessis*, Gay-Lussac, Hauy, &c.

Among the private lecturers Esquirol, Alibert, Magendie and Broussais, are the most distinguished.

No student is permitted to offer himself for examination, without being able to exhibit the sixteen certificates of inscription, given out during the course of instruction. If, during his attendance, he has forfeited any of these certificates, he must continue his attendance until he has obtained the full complement, before he can be admitted to an examination. The candidate for a degree has to undergo *six examinations*. These are conducted publicly, and each lasts one hour. After having passed successfully through his examinations, the candidate hands in his *dissertation*, which he defends publicly, as is customary in the colleges of other countries.

It is manifest, says our author, from a view of the course of studies designated in the above table, that, in France, the interests of surgery predominate greatly over those of medicine.—Five seasons are devoted to anatomy; two to *external* or surgical pathology; two to operative surgery; three to surgical clinics, and one to midwifery; whilst every thing which relates to the study of internal diseases is

limited to two seasons only; nay the *Officiers de Santé*, are required to attend but one season to these studies. No lectures are delivered on general pathology; and what is more remarkable, the lectures on *internal* nosology are limited to two courses.

We pass on to the fourth chapter, which is devoted to an account of the infirmaries, dispensaries and hospitals, located in the French metropolis. The number of these institutions in Paris is very great. More than 35,000 patients are annually received into them.

The *Secours à domicile*, of which there are twelve in Paris, are public charitable institutions for the relief of the poor and sick at their houses. These dispense food, clothing, wood, &c. to the poor, and supply them with medical attendance and medicines. Besides these public *charities*, there are a great number of private associations, by which medical and other relief is afforded to the poor The *Societé Philanthropique* alone supply medical attendance, &c. to about 2000 patients annually.

Maisons de Santé; these are not properly speaking charitable institutions, although their usefulness in so populous a city must be very extensive. They are designed as private hospitals, in which, persons who have no regular homes, or who want the conveniences of proper attendance, may obtain them at a moderate expense. The author counted fifteen of these institutions. They are all located in the remotest suburbs, and are generally environed by pleasant gardens, and removed from all tumult and noise.

The *Hospices*, resemble our alms-house infirmaries. There are 12 of these in Paris. In 1819, the number of invalids received into these *Hospices* amounted to 18,260.

Paris contains ten *Hospitals*, properly so called, all of which derive their revenue from government. They are, in fact, integral parts of the government, and are placed under a *central administration*, which exercises a general superin-

tendance, not only over the hospitals and *hospices*, but also over the above named benevolent institutions.

The *Bureau central d'admission*, which is under the immediate direction of four physicians, is as it were the initial point of the hospitals. Here *all* must apply, who wish to gain admission, as patients, into a public hospital. The central administration determine according to the kind and degree of the disease, to which of the hospitals the applicant shall go. The central administration is always accurately informed of the number of disposable beds in these institutions; for the local superintendants of each hospital are obliged every morning, after the physicians have been round the wards, to render an exact account to the *central administration*, of the number of patients, of unoccupied beds, of deaths, admissions, discharges, &c. &c. The central Bureau is itself an institution, in which medical advice and medicines are given gratis to the poor.

In the Parisian hospitals the sick are separated into classes, according to the general character of their diseases, and placed in distinct wards. They have also their (*Salles de Medicine*) medical wards, and (*Salles de Chirurgie*) surgical wards. Several of the hospitals are exclusively devoted to the reception and treatment of certain diseases, and which, with but very few exceptions, are not admitted into the other public institutions of this kind. Thus, venereal patients are admitted only to the *Hôpital de Capucins*. Those affected with chronic cutaneous affections, are sent to the *Hôpital St. Louis*. Children to the *Hôpital des enfans malades*. Epileptic and maniacal patients have their appropriate institutions; and those affected with colica pictonum, and consumption, are for the most part sent to *La Charité*.

The attending physicians and surgeons make their visits daily at six o'clock in the morning during summer, and at seven o'clock in winter. They enter the wards, dressed in long white aprons, and begin by calling out the names of those

(eléves) pupils who are attached to the institution, and whose duty it is to attend them in their visits round the wards. Two of the *eléves* are appointed to the task of recording the dietetic and medical prescriptions in separate books, as they are dictated by the prescribing physician at the bed side. The physician then signs his name at the bottom of the day's prescriptions, without which no attention would be paid to them by the Apothecary. In general the hospital practice, in the medical departments, consists of a routine of passive medical and dietetic treatment, very different from the active and often bold practice pursued in the English and German hospitals. On looking over the prescription book of a Parisian hospital, we might suppose that the words, *saignée*, *sangsues*, *decoction blanche*, *eau d'orge*, *tisane de bourrache*, *de chiendent*, *de réglisse*, designated nearly their whole materia medica.

The physicians and surgeons attached to the hospitals succeed each other every two months in their duties. Our author very properly condemns this regulation, as unfavourable at once to the interests of the sick and of the pupils who resort to these institutions for instruction. The difference in the views and doctrines of the attending physicians is often perniciously exemplified in these changes, and well calculated to fill the student with an injurious scepticism in relation to the foundations of medical science. It not unfrequently occurs, says Dr. Casper, that a phlebotomizing disciple of Broussais is succeeded by a *hœmoptophobe*, who declaims violently against the diagnosis of an obscure inflammation, and hurries on to a vigorous diaphoretic treatment, overwhelming his patient with valerian, angelica, &c.

The pupils (*eléves*) immediately attached to the Parisian hospitals, of which there are two classes, the (*eléves externes*) out door pupils, and the (*eléves internes*) resident pupils, are chosen in the following manner: In November of every year the *Administration géneral des Hôpitaux* hold a "*concurs*,"

to which all matriculated students between the ages of 18 and 24 are admitted. A sworn committee of two hospital physicians and three surgeons, chosen by lot, act as umpires in these probatory exercises of the students, and nominate from among those who have acquitted themselves successfully, the *eléves externes*, wanted to fill the vacancies in the different hospitals. In these examinations (*concurs*) the students receive by lot certain *theses* or propositions in anatomy, physiology and surgery, for extemporaneous, as well as others on the same branches, for written answers. Eighty are chosen annually; these are distributed among the hospitals in the proportion of *four eléves* to every 40—50 surgical patients, and *two* for every 60—70 patients in the *medical* wards. These pupils do not reside in the hospitals, and receive no pecuniary remuneration whatever. Their duty is to accompany the attending physicians and surgeons in their morning visits round the wards, to visit the patients again in the evening; to bleed, cup, leech, dress blisters, apply bandages and other dressings, &c. &c. The peculiar advantages of an *eléve externe*, besides the opportunities of practical instruction which he enjoys, consist in his being permitted, after having served one year, to "*concur*" again for the situation of an *eléve interne;* a post which is much sought after on account of its scientific and pecuniary advantages. For this purpose, a second "*concurs*" or examination is annually held, for determining which of the *externes* are to fill the vacant situations of the *internes* or resident pupils. Twenty of these are chosen every year, and retain their places for four years. The duties of a resident or house pupil are considerable. He has the immediate care of from 40 to 50 patients—superintends the prescriptions of the attending physician—keeps the records—collects observations—reports to the prescribing physician the condition of the sick on the preceding evening—is concerned in the (*bureau de reception*) the reception of new patients—prescribes himself in urgent cases—makes the dissections, &c. &c. One or two *internes* remain

day and night in the *chambre de garde*, an apartment kept open in the large hospitals for accidents. These pupils live in the hospitals—get their boarding, and receive, besides, several hundred francs yearly.

We shall now proceed with our author, to notice the principal Parisian hospitals. The *Hospital St. Louis* is one of the most remarkable and interesting of these institutions to the physician. It is almost exclusively devoted to patients affected with exanthematic diseases; and contains 1,100 beds.

Alibert, who stands among the first physicians of the present age, is at the head of this hospital. It is in this extensive receptacle of disease that he has collected those observations which form the materials of his splendid work on *cutaneous diseases*. Here the whole family of this loathsome class of maladies is assembled. From 900 to 1000 patients of this kind are constantly found in this institution, and in this respect, it may with propriety be called, as Dr. Casper observes, *l'égout de toutes le contrées du monde*, the common sewer of all the countries on earth. Under this head Dr. C. introduces some judicious observations on the comparative excellence of Bateman's and Alibert's classification, and, in common with the majority of his German brethren, gives a decided preference to the former.

Alibert's cardinal remedy in chronic affections of the skin is sulphur. He employs it in almost every instance in some form or other. The use of baths, too, is carried to an almost incredible extent in this hospital. The bathing establishment is calculated to accommodate patients *living without* the hospital, as well as those who are its inmates. One hundred and fifty thousand baths are said to be allowed annually to patients living in the city;* and besides these, the number of

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* As an example of the spirit of speculation among the Parisians, Dr. C. observes, that he saw over a small tavern opposite the hospital the following sign: *Ici on loue des lits aux personnes qui prennent des bains dans l'hôpital!*

patients treated in the hospital, averages 9000 a year, nearly all of which take at least one bath daily.

The sulphur vapour-bath, introduced by *D. Galés* is now almost entirely confined to the cure of itch. Biett, one of the physicians of this hospital is fond of using mercurial fumigations in inveterate syphilis. The Russian vapour-baths, too, are much employed by this physician in "rheumato-arthritic affections."

Purgatives also are a favourite remedy in this institution; and Alibert speaks in the highest terms of the utility of the extracts of *fumaria*, *trifolium*, *taraxacum*, &c. "What is more peculiar, however, in the practice of this physician, is his frequent employment of a strongly, external stimulating treatment in cutaneous affections; and this is the more remarkable, as the English, for the most part, advise a contrary treatment. In almost every lecture, Alibert says, "we must excite a local fever." With this view he directs his patients to be bathed with a solution of the nitrate of silver; and in very obstinate herpetic affections he often applies an arsenical paste. Frictions of the affected part, while in the bath, with a flesh brush, is also a frequent prescription in this hospital. The cure is almost invariably ended, in Alibert's patients, by the use of an opiated white precipitate ointment *(pommade mercurielle opiacée.)*

Alibert thinks, that when patients die of chronic cutaneous diseases, it is in consequence of the mucous membrane of the bowels becoming inflamed, from the sympathy which subsists between this membrane and the external surface, and by which a colliquative diarrhœa is produced.

Our author here introduces short sketches of the characters of Alibert, Biett, and Richerand. Alibert is not above forty years of age. In his delivery, he is off-hand, rhetorical, rapid, lively, witty, and more diverting and interesting than instructive. In his exterior, as well as in his conversation, he is altogether the Frenchman. He recommends, in the same breath, the study of the Arabians, and a new work on physi-

ology—produces a patient and a drawing from his splendid work—throws the prospectus of a new work among the audience, and distributes a sample of a new refreshing beverage among the pupils; and whilst they are tasting around, spices his already too piquant discourse, with witty anecdotes, and *bons-mots.*

Biett is a young man, of great merit. "If it be pleasing," says Dr. Casper "to eulogize what is praise-worthy, I enjoy this pleasure in the most lively manner in speaking of this interesting and ingenious young man." Not one of his colleagues is so deeply interesting and instructive in their hospital visits as Biett. He is extremely collected, earnest and shrewd in his clinical examinations, and, by consequence, generally very accurate in his diagnosis and curative indications. He is obliging in his manners, fond of discussion, and zealously devoted to his profession.

Hotel Dieu. This hospital is situated in the centre of the most populous, noisy, close, and dark quarters of Paris. It contains twenty-three large halls. It has 1300 standing beds, with a considerable number of *lits suplémentaire,* for occasions of emergency. All classes of diseases, except the diseases of children, syphilis, itch, and mental maladies, are received into this institution. *Dupuytren* is at the head of this hospital. He is undoubtedly the first surgeon in France. His clinical lectures are truly interesting and instructive. He is not above thirty-seven years of age, and possessing an acute and vigorous understanding, great industry and zeal in the pursuit of his profession, and a most extensive field for experience, we have reason to hope important accessions to surgery from the future course of this active and intelligent surgeon. His delivery is graceful, and his language strong and perspicuous.

There is, however, one trait in his character, which is by no means an amiable one. In his deportment towards his patients and his pupils, he is often extremely unkind, cold, and peevish.

Having given a sketch of the character of this eminent surgeon, our author next describes the manner in which he goes through his hospital duties. After having been round the wards, he repairs to the *operation-hall*, followed by a great number of *eléves externes* and *internes*, and a large crowd of other attendants at the hospital. Here he delivers instructive remarks on the new and interesting cases that may occur in his wards, gives the results of autopsic examinations, exhibits specimens of morbid anatomy, and finally proceeds to perform such operations as may be required at the time. Dupuytren operates with uncommon neatness, despatch, and certainty.

The author here introduces short accounts of some of the principal operations which he witnessed by this surgeon.

In a case of polypus of the nose, of the fibrous kind, the extraction of which had been several times unsuccessfully attempted, Dupuytren, having first introduced an elastic sound through the nose into the fauces, and fastened to it a dossel of lint, slit up the wing of the nose on the affected side, (according to the old plan,) and then seized the polypus with a forceps, and extracted it completely.

The author states that Dupuytren performed the operation of lithotomy *five times* in the course of two years, for the purpose of extracting broken-off-ends of catheters that had slipped into the bladder. He has extirpated twelve cancerous uteri. In two instances only, however, did the patients obtain complete relief. In one case the patient died under the operation.

The *physicians* belonging to this extensive hospital, are Recammier, Husson, Petit and Montaigne. They are all more or less attached to the doctrines of Broussais; Husson especially is a warm partizan of Broussais. With him, consequently, every thing is *gastro-enteritis*, and leeches are a principal means of treatment. Recammier, who has the appearance of an old and calm man, is in reality a lively, imaginative, and in some respects an original spirit. He possesses much learning, and is esteemed one of the first physicians

in Paris. In accuracy of diagnosis there are few who are his equals.

La Charité. This hospital contains but 800 beds, 104 of which are appropriated to surgical patients. It is, notwithstanding, a very interesting clinical institution, on account of the eminence of several of its teachers. *La Charité* is remarkable for the number of pulmonary diseases, and of *colica pictonum* which are constantly brought to it. Consumption, indeed, appears to be an exceedingly common disease in Paris. M. de Chateauneuf, in a statistical paper presented to the French Academy in 1820, has shown that out of 64,441 deaths in Paris, during the years 1816, 1817, 1818, the number and proportion of pectoral diseases were as follows: 604 asthma, 1894 inflammation of the lungs, 4259 catarrh, 6971 phthisis pulmonalis. According to this statement, therefore, the proportion of deaths from asthma was nearly as 1 to 100, of pneumonia as 1 to 83, of catarrh as 1 to 15, and of consumption as 1 to 9.

The treatment of pectoral diseases in *La Charité* is exceedingly simple and inactive. Great attention is paid to the appearances and character of the *sputa*, and the thorax is examined according to Laennec's method with the utmost care; but the therapeutic management is feeble and temporizing; consisting often of little more than the exhibition of pectoral syrups;* ptisans, and occasionally a little *kermes*. Indeed the whole medical treatment in *La Charité* consists of a *stereotype* routine, which has been pursued from time immemorial with the most pertinacious exactness.

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* Of these the following are the most frequently employed in this institution.

Sirop pectoral de Chaussier.

℞ Kali sulphurati	-	-	-	℥jss.
Sacch albi	-	-	-	℥x.
Aq. Hysop	-	-	-	℥vj.

f. l. a syr. a tea-spoonful mornings and evenings in chronic-inflammatory pectoral affections.

The medical gentlemen attached to this hospital are *Boyer*, *Roux*, *Lerminier* and *Fouquier*. Boyer is already considerably advanced in years; and on account of his infirm state of health, is not often seen at the hospital. As a writer and practical surgeon, Boyer is deservedly placed in the first rank of French surgeons. As a public teacher, however, his merits are far from being great. Both in his conversations at the sick bed, and in his public lectures, he is intolerably monotonous and dull. *Roux*, his son-in-law, is a young man of very considerable learning and talents. He possesses an enterprizing, prompt, and lively disposition, and is ardently devoted to his profession. He is very popular as a teacher. *Lerminier* has charge of the principal wards of *La Charité*. In his treatment of diseases he is a most inveterate *routinist*. With a small number of ptisans, and *magistral-formulæ*, he generally pursues a mere temporizing palliative course, and seldom directs his diagnosis beyond the more obvious phenomena of diseases. He is kind and familiar towards his pupils, but his lectures are for the most part little instructive. *Fouquier* is one of the most esteemed Parisian physicians; few of his countrymen equal him in the accuracy of his diagnosis. He examines with great care, before the pupils, such cases as come under his notice in the hospital, as well as the reports which his pupils are obliged to draw up of particular cases, and criticises them with earnestness and severity, where severity is in place. Fouquier's practice is peculiarly remarkable for the enormous doses of powerful medicines which he pre-

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Sirop anti-asthmatique.

℞ Ammoniaci depurat - - ℥jj.
Macer cum, vini gallici alb. - ℥vjjj.
adde
Sacch - - - - libj.

f. l. a syr. Dose from ℥j. to ℥j, three or four times a day.

Pilules anti-catarrhales de Pariset.

℞ Tart. antimonii; extract. opii. aa. gr. jjj. G. Tragac. gr. x. Cons. rosar. q. s. f. l. a. Pill. No. 60. S. two mornings, and two evenings, in chronic catarrhs.

scribes, as of opium, conium, nux-vomica, sugar of lead, &c. As a teacher of clinics, Fouquier is unquestionably the most interesting and instructive of all his colleagues.

We pass over our author's accounts of the hospitals, *des Veneriens, des Enfans Malade, de la Pitié, Necker, Beaujon, Cochin, du Val-de-Grace, and Hôpital Militaire de la Garde Royale.* In the *hôpital St. Antonine*, the following remedy is much used, and with considerable success, in chronic, mucous, and sanguineous discharges from the bowels.

℞ Rad. Ratan. - - ℥j.
Spir. Vini alcohol - ℥jv.
f. l. a. Tinct. S. gtt. xjj. to ℥j. daily.

Beauchesne, principal surgeon of this hospital, has lately made some interesting cures of cancer without an operation. Our author mentions a case of this kind, in which he had an opportunity of witnessing this physician's mode of treatment, and its effects on the disease. A woman aged sixty-five, affected with cancer of the nose, was received into the hospital. The disease had commenced two years before; the right wing of the nose, and the whole right side of the upper lip, was deeply ulcerated, being attended with excessive pain. The treatment was as follows: The ulcer was daily washed with injections of a decoction of cicuta, in which corrosive sublimate was dissolved in the proportion of two grains to ten ounces of the decoction. In addition to this, the patient took large doses of cicuta internally with small doses of corrosive sublimate. After four month's continuance under this treatment, the lip was entirely healed, except a point about the bigness of a pin's head at the angle of the lip.

The fifth chapter of this work is devoted to a particular account of the practice pursued in the Parisian hospitals, in the treatment of certain important diseases.

Epilepsy.—The hospitals *Salpetriére* and *Bicêtre* admit more epileptic patients than all others put together. A large proportion of these patients are affected, at the same time, with mental disease; and are received more for the purpose of

affording them an asylum than with a hope of curing them. Out of 250 epileptics, which our author saw in the hospital *Bicêtre*, Pariset selected but nineteen as fit subjects for remediate treatment. This physician is very fond of employing the *actual cautery* in epilepsy. He applies it upon the vertex of the head; and thinks that he has done more good with it than with any other remedy. Esquirol, also, in the hospital Salpetriére, has employed the actual cautery in this way, with some advantage. He puts no confidence whatever in the employment of the metallic oxydes in this disease.

Biett, one of the physicians of *St. Louis*, on the contrary, considers the *nitrate of silver* as an important remedy in this affection, and is said to have employed it in several instances with complete success. Dr. Casper saw an epileptic in this hospital, who had taken eight grains of nitrate of silver daily, for two years; his skin had acquired a dark colour, like that of an American Indian. The paroxysms which at first returned daily with great violence, became less severe, and recurred only every month. Besides this remedy, Biett is fond of giving the essential oil of valerian, to patients labouring under this disease, in doses of from forty to fifty drops. He also employs *spir. terebinth* in drachm doses; and the *animal oil* of Dippel, to which latter article he is particularly partial; and which he administers in doses of 150 drops twice daily.

Itch.—The root of the *plumbago Europea*, which was introduced into regular practice by Sumeire, is frequently employed both in private and in hospital practice, and is by many regarded as among the most efficacious remedies in this disgusting disease. It is employed in the following way: Take three handfuls of the root, and one of common salt; beat them well together in a mortar, then pour on it one pint of hot sweet oil. Strain off the oil and tie up the residuum in a little linen bag. In using it, the bag must be dipped in the warm oil, and with it, the whole surface of the body rubbed every morning and evening until the disease has dis-

appeared. Three or four frictions are said to be sufficient in ordinary cases. The *aunus nigra baccifera* is used in the same way; and *Percy* asserts, that he has never known it to require more than ten days to cure the itch with this article.

Alibert is much in the habit of employing an ointment composed of two parts of *flor. sulphuris*, one of carbonate of potash, and eight of lard. Frictions with this ointment are made three times daily. *Dupuytren*, in his practice in the Hotel-Dieu, employs a wash made by dissolving four ounces of kali sulphuretum in three half pints of water, and adding half an ounce of sulphuric acid. The patient is directed to wash himself with this. *Chaussier* recommends his *savon mercuriel*; it consists of a combination of three ounces and a-half of mercurial ointment, and three of a caustic solution of soda. This compound is said to be very efficacious in other hepetic eruptions, and in different species of psora.

Arsenic, though not a new remedy in chronic diseases of the skin, is, perhaps, no where so much employed in inveterate cases of this kind, as in the hospital *St. Louis*. Biett, especially, places much confidence in its powers. In that species of herpes *(herpes exedens)* which sometimes attacks the nose of scrophulous persons, *Alibert* invariably applies frére Cosme's* paste to the ulcer.

It is however chiefly as an internal remedy that arsenic is extensively employed in this hospital. *Biett* prefers, for this purpose, the *arseniate* of *ammonia*. This preparation is made by dissolving one part of arsenic in four parts of nitric, and a-half of muriatic acid. The solution is then saturated with carbonate of ammonia, and evaporated. The product thus obtained is the arseniate of ammonia. One grain of this salt dissolved in an ounce of distilled water, with the addition of

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* It is thus made: Take two drachms of cinnaber, eight grains of the ashes of burnt shoe soles, twelve grains of dragon's blood, and forty grains of white arsenic: reduce them to powder, and make it into a thin paste with water.

a little spir. lavend. is the usual form for exhibiting this remedy. The dose is from 25 drops to a drachm daily. The *Asiatic pills*, also, the formula of which was first published in the Asiatic Researches, are frequently employed in this hospital.* In this place the author introduces short notices of several inveterate cutaneous affections which were cured with this remedy, during his attendance at the hospital. A man aged forty, who had been affected for several years with a severe herpetic eruption of the scalp, and in whom the disease had already given rise to occasional symptoms of mental derangement, was perfectly cured in three months by the above solution. He saw, also, two cases of *herpes furfuraceous*, cured with this remedy in a short time. In one of these cases but three grains of arsenic, and in the other but five were used. A case of impetigo *(dartre crustacée humide)* resisted this remedy for a long time; finally, however, it yielded to its powers.

Tinea.—The treatment of Tinea in the Parisian hospitals, consists almost entirely in the use of *an arcanum!* Two brothers of the name of *Mahon* are in possession of a secret remedy, which, by an order of government, is employed under their immediate direction, in all the hospitals. For this service they are allowed one thousand francs yearly, and six francs besides for every patient cured. Of 795 children, 527 were, in the space of five years, radically cured of tinea by this *arcanum.* This remedy consists of a powder and an ointment which our author thinks contains *carbonate of soda.*

Alibert's external remedies in tinea, consist almost exclusively of ointments composed of lard and pot-ash,—or of lard and powdered charcoal,—or finally, of lard, pot-ash, and charcoal. Internally, he frequently directs emetics and pur-

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* These pills consist of one-seventh of a grain of arsenic, and six-sevenths of a grain of black pepper. One grain of this mass is taken at a dose.

gatives; the expressed juice of *trifol. fibrinum, borrago,* decoct. bardanæ, flowers of sulphur, crocus martis.

In England, the oil of turpentine has been lately employed in tinea, with considerable success. The head is bathed with a mixture of one ounce of spir. terebinth, and two drachms of ol. olivar.

Syphilis.—In the practice of the Parisian hospitals, as every where else, mercury constitutes the principal remedy in the treatment of this disease. A solution of corrosive sublimate in alcohol, is the usual form for exhibiting this remedy. In addition, however, to the employment of this active mineral, *diaphoretic ptisans* are very commonly administered in this disease. The "*Tisane de Felz*" is a favourite prescription in the hospital *St. Louis.* It is made according to this formula.

Stibii sulphurati nigri - - -	ʒxj
Rad. Sarsaparill.	
Chinæ - - -	āā ʒvj
Herb. Hederæ ter	
Ichthyocollæ - - -	āā ℥ss
Coq. cum aquæ lib. iv ad Col. lib. jj adde	
Hydr. muriat. corros. - - -	gr. j
S. M. To be taken in the course of 24 hours.	

Gold, according to our author's account, does not sustain the reputation it had acquired as a remedy in this disease. It is still, however, employed by some Parisian physicians, in inveterate cases of this kind, especially where mercury has failed to perform a cure.

Cullerier has made some experiments with the *muriate of platina,* in syphilis.* He selected seven patients for this purpose. One of these had small ulcers on the glands penis, which could not be certainly pronounced syphilitic. The *muriate of platina* was prescribed, of which he took four and a third grains in the course of twelve days, by which

* Dict. des Sciences Med. Art. Platine, 1820.

time all the symptoms were gone. A second patient had phymosis and bubo. He took five and a half grains of this preparation in the course of five weeks, and was cured. A third patient, who had venereal excrescences, received no benefit from this article, although he took ten grains. A fourth one was affected with painful exostoses; he took ten and a half grains, by which he was entirely relieved from the pain; the tumours, however, were but slightly diminished. A fifth, had gonorrhœa, chancre, and bubo. He took the platina ten weeks. In eight weeks the chancres and bubo were healed. A sixth patient had syphilitic eruptions. He took nineteen grains, but was only partially freed from his disease. The seventh case was similar to the preceding one; no radical cure was effected in this instance by the platina.

Injections are not commonly employed for the cure of gonorrhœa, in the French hospitals; and in the *hôpital aux veneriens*, primary chancres are seldom touched with caustic. In the deep secondary ulcerations of this disease, however, Cullerier applies the butter of antimony; and in the hospital *St. Louis*, such ulcers are cauterized with nitric acid.

We pass over our author's description of Laennec's stethoscope, and proceed to his account of Fouquier's "*mode of employing sugar of lead in pulmonary consumption.*" Sugar of lead is an old remedy in this disease.* But the great extent to which its internal use may be carried, without producing deleterious effects in the system, is not very generally known. According to the experience of Fouquier, *sugar of lead* possesses a *specific* power against the colliquative sweats of phthisis. He denies that it can produce colica pictonum; an assertion which we are, however, by no means disposed to receive as correct. We have

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* In the Pharmacopœia Bateana, published in 1713, there is a formula for a *Tinctura Antiphthisica*, which consists of a solution of sugar of lead and *sal-martis*. It is stated "to be truly a good medicament in those consumptions which proceed from ulcers of the lungs."

ourselves seen several cases of lead cholic, which were, unequivocally, produced by this preparation. It is nevertheless certain, that it is much less apt to give rise to this affection than is generally supposed. Fouquier asserts that it may be given to the extent of twelve grains daily, without danger. The medium portion *per diem*, is from four to eight grains. In employing it in consumption, its use should be continued as long as the colliquative sweats return. Dr. Casper saw thirteen cases treated with this medicine in *La Charité*. Four of these were cured, five died, and four were much relieved. Dr. C. here introduces an account of several cases of this disease, in which the beneficial effects of this preparation were conspicuously demonstrated.

"A man aged forty-seven, far advanced in phthisis, and greatly exhausted by excessive colliquative night sweats, was put on the use of the sugar of lead, on the 20th of September. He took one grain daily, until the 24th, by which time the sweats were considerably lessened. He continued to take this quantity daily, until the 29th, when he was entirely freed from his exhausting night sweats. On the 4th of October, however, the sweats returned. He now took six grains daily, until the 10th, when they were again completely suspended; this dose was nevertheless still continued, until the 16th, when it was increased to eight grains, daily. There were no night sweats. On the 18th, moderate perspiration in the morning; on the 19th, no sweat. The last dose was continued. On the 20th, the daily dose increased to eleven grains. No sweat. By the end of the month the patient was well, and left the hospital.

"A woman aged thirty-three, who had unequivocal symptoms of phthisis, and who suffered, especially, from exhausting night sweats, took, on the 27th of August, three grains of sugar of lead. On the 29th, she took seven grains, which had the effect of diminishing considerably the colliquative sweats. On the 30th and 31st, she took eight grains; the sweats, however, again increased. September 1st, took nine grains; sweat diminished. On the 2d, took ten grains, which put an entire stop to the sweats. The medicine was continued a few days longer; and on the 15th, left the hospital entirely relieved from this distressing symptom."

"A young woman affected, for eighteen months, with a catarrhal affection, attended with profuse and debilitated night sweats, was put on the use of the sugar of lead, on the 18th of August. After she had gradually increased the dose to nine grains, daily, the night sweats were considerably lessened. On the 26th, no sweats. She now became affected with violent colic, attended with diarrhœa. The medicine was discontinued for a

few days, but as the night sweats again appeared, she took, on the 12th of September, four grains of the lead. 13th, took six grains, no colic, and but little sweat during the night. 16th, dose increased to seven grains, which was continued. The sweats returned no more; and the patient was discharged by the end of the month."

The author goes on to relate the circumstances of other similar cases; all of which undoubtedly go to show that *sugar of lead* is a valuable remedy against this one symptom,—*colliquative night sweats.* The author does not say, however, whether, in other respects, there was any improvement effected in the disease. The mere removal of this symptom will no doubt make the patient much more comfortable, and economize his strength; yet we cannot presume that it would strike at the root of the disease,—that it would, in fact, cure the local pulmonic affection.

Colica Pictonum.—We never find the hospitals *La Charité* and *Hotel Dieu*, without a considerable number of patients affected with this disease. The treatment pursued is a settled *routine* of *prescriptions*, which are punctiliously adhered to in these institutions, and which is familiarly known by the title of *traitement de la charité.* When a person labouring under this disease, is admitted into any of the hospitals, the first prescription is the (*lavement purgatif des peintres*) the painter's purgative enema, viz:

"℞

Folior. Sennæ	- -	℥ss
Coq. c. aq. comm. adde	- -	℥xii Col. vjjj
Sodæ Sulphat.	- -	℥ss
Vin. Antimonii	- -	℥iv

S. M. To be used as an enema.

"During the day the patient must drink the *eau de casse avec les grains*, namely:

"℞

Cassiæ fistulæ	- - -	℔j
Coq. c. aq. comm. l. ad Col.		℔jj
Tart. natronat.	- -	℥j
——Antimon.	- - -	gr. iii

S. M.

"In the evening the (*lavement anodin,*) anodyne enema is administered, made of nut oil, ℥iv and red wine ℥xii. Three hours after this, three drachms of theriac, with a grain and a half of opium, are given.

"On the following day, the patient gets the *eau bénite,* according to the following formula:

"℞

Tart. antimon. - - - gr. vi
Aq. destil. - - - - ℥viii
M. The half to be taken directly, and the remainder in an hour after.

"If this produce vomiting, the patient is put on the use of the following ptisan:

"℞

Liq. Guaiaci
——Sassafras
Rad. Chinæc
——Sarsaparilla - - āā ℥j
——Liqueritiæ - - ℥ss
Coq. l. a. c. aq. lib. jjj ad col. lib. jj

"In the evening, the *lavement anodin* is repeated. On the third day the patient drinks two pints of the above ptisan, in which one ounce of Fol. Sennæ is previously boiled. In the evening the first enema (*des peintres*) is repeated, which is followed in two hours by the second enema (*l anodin*) and the dose of theriac and opium. On the fourth day, the patient commences with a new medicine,—*purgatif des peintres,* viz:

"℞

Fol. Sennæ - - - ℥jj
inf. c. aq. q. s. ad colat. - ℥vi
Soda Sulphat. - - - ℥ss
Rad. jalap - - - ℥j
Syr. Spinæ Cervin. - - ℥j
S. M. During the day, the above sudorific ptisan.

"On the fifth day, the *tisane sudorifique laxative* is continued, and in the evening, the first enema, followed by the second anodyne one, with the dose of theriac and opium are administered.

"On the sixth day, the above *purgatif des peintres;* the simple sudorific ptisan,—then, in the evening, the two enemata, with their consequent dose of theriac and opium. Commonly the disease disappears after the second purgative; should the pain, however, continue, the strong purgatives are to be exhibited, until the disease yields. If, notwithstanding, the bowels remain too constipated, and the patient do not vomit and purge, he must then take the following active cathartic:—(*bols purgatifs des peintres.*)

"℞
Aloes soccot
Extr. Rhæi - - - āā gr. x
Jalap
Hellebor. nig.
Scammon - - āā gr. xiv
Syr. Spinæ Cervin. q. s. x. boli No. vi. one every three hours."

This, then, is the fixed *stereotype* course pursued in the treatment of colica pictonum in the Parisian hospitals. PINEL very justly calls this practice an *empirisme consacré par une longue suite de succés;* and it must be confessed, observes our author, that the success attending this treatment has not been small, since, out of 1353 patients, treated in this way, in *La Charité,* during twelve years, only 64 died. P. 258.

We cut short our review of this interesting work, in this place, reserving the remainder of the volume for notice in our next number. E.

ART. IV.—*Principles of Medical Science and Practice.* Part I. *Physiology.* By HARDWICKE SHUTE, M. D. Physician to the General Infirmary, and to the county and city Lunatic Asylum, Gloucester. Octavo, pp. 566. London, 1824.

IT was not said without foundation, that "of making books there is no end, and much study is a weariness of the flesh." In our occupation we are often obliged to wade through some half score of new publications, before we can bring together as many interesting facts, or even opinions, as may serve for the commencement of a review. It seems to have become the fashion of this book-making age, to write with prolixity instead of brevity or perspicuity. An author catches up a new idea, and before it gets out of his hands, it becomes enveloped in a whole octavo of every day matter. No where is this more generally the case, than in our own mother country, of whose liveliest author it was once observed, that "he could

lecture all day upon a broom-stick." But the physicians and surgeons of Britain, especially, appear to have acquired the habit of which we are forced to complain. Many of their publications are made to cover pages altogether beyond the proportion of ideas which they convey; and the size as well as expense of new medical books has, of course, become too exorbitant for the general class of readers.

It was with a view to these very circumstances, that we were first induced to undertake the charge of this journal; inasmuch as by condensing the current knowledge of the day within some reasonable limits, we hoped to bring every thing of a novel or interesting character before the eyes of our readers. The work now under review, however, has proved too serious an impediment to be leaped over by the most zealous efforts. We have laboured harder and longer to give it what is usually called a full reading, than we have generally been obliged to do upon any single publication; and now that we have examined it from beginning to end, we find it difficult to revert to any part from which our pages can derive much interest. The author is a very sciolist, who often quotes names and opinions for authority, instead of resorting to facts or to reason. He is evidently a closet philosopher, who undertakes to establish his own views by mere speculation, while he boldly assails the opinions of such writers as Philip and Le Gallois, without adducing the authority of a single new experiment. Still, however, even an author of this character may occasionally throw out ingenious ideas respecting circumstances which others would have passed over with less examination. To do justice to Dr. Shute, we must confess that we have found this to be the case in several parts of his book; and from the turn of mind which he evinces for elaborate discussion, we have no doubt that he will appear to much greater advantage in the practical part, with which we suppose the second volume will be occupied. In another respect, also, we feel constrained to allow no small degree of credit to our author. He has brought together ma-

ny interesting facts and opinions, which in a detached situation would have excited no particular notice; and in some places he supports his peculiar views with considerable force and ingenuity. We will undertake, therefore, to review the book, notwithstanding the unfavourable opinion we have expressed of its general merits, and of the unreasonable size to which it has been extended.

The great object of our author, as far as we can discover it in the preliminary essay to this physiological part of his work, is to prove, that all the functions of the body are of a *digestive* character. The introduction of food into the stomach is not for "its conversion into chyle, or even into blood; it is intended to constitute a component part of the animal frame." He thinks, therefore, that "the conversion of certain parts of the *blood* into the component parts of the body, is accomplished by a process similar to that by which certain parts of our food are converted into chyle,"—in other words, that it is digestion; and that the process of digestion is not accomplished until "certain parts of our food are converted into the component parts of the animal frame." He intends to show that "the different functions of digestion, absorption, assimilation, circulation, nutrition, secretion, and excretion, all constitute one extended system of actions, by which our food is in reality digested." He thinks that the removal of those parts from the system which are not calculated to enter into its composition, is also effected by a digestive process; and upon these grounds he has "ventured to adopt the unusual expression, that the blood is digested." Respiration he also makes out to be a digestive process, by means of which the "ærial influence" is digested into the "arterial influence," which in its turn becomes afterwards digested into the "nervous influence." The air, like the food, he therefore supposes to enter into the body as a component part of its organization. "If the ærial influence cease to constitute a component part of the organization," he conceives "that that organization ceases to be endowed with the principle of life, and

therefore ceases to be endowed with the susceptibility of action, &c."

In relation to these propositions, it may be observed, that they do not differ essentially from the opinions commonly entertained by physiologists. The only important difference consists in the application of the term digestion, to which our author has allowed a much more extended sense than has been hitherto accepted by the profession. It may be questioned, however, whether such a latitude in the use of terms is calculated to produce any good effect in scientific discussions. We have always found it best to call things by their right names; and cannot approve, therefore, of this grouping together several different processes of the animal economy under one appellation, merely because they happen to resemble each other in some imaginary shades of character.

The body of the work is comprised in eighteen chapters, the subjects of which are as follows:—1. *Digestion,* (properly so called)—2. *Absorption* —3. *Assimilation.*—4. *Respiration.*—5. *Circulation.*—6. *Nutrition.*—7. *Secretion.*—8. *Excretion.*—9. *Electricity.*—10. *Nervous Influence.*—11. *Living Principle.*—12. *Living Action.*—13. *Locomotive Functions.*—14. *Digestive Functions,* (Respiration)—15. *Digestive Functions,* (Circulation!) 16. *Digestive Functions,* (Nervous action!) 17. *Digestive Functions,* (Animal heat!) 18. *Sensitive Functions.*

It is in only a few of these chapters that any novel or very interesting matter is contained. Although we shall, therefore, run them over hastily, we will not, however, neglect to detail, as far as possible, every thing that can instruct our readers.

Under the head of digestion, we have not been struck with any thing of a peculiar character, except, perhaps, the opinion that the formation of chyle is completed in the stomach. He grounds this opinion chiefly on the fact, that Mr. Hunter once detected a *whitish fluid like cream* above the pylorus in some dogs, upon which he was experimenting for the pur-

pose of ascertaining whether the gastric fluid was of an acid or alkaline nature. After controverting Sir Everard Home's statement respecting the hour-glass contraction of the stomach, on the authority of "several experimentalists," whom, by the way, he does not name,—he proceeds as follows:

"There is nothing in the opinions detailed, which does not appear to justify the conclusion, that the food, in consequence of its admixture with the gastric fluid, and of the living action of the stomach, is separated into certain elementary particles, which are capable of re-uniting in those peculiar proportions which constitute chyle. The food thus changed by the action of the stomach, consists, as before stated, of certain particles united, or capable of uniting, in those proportions which constitute chyle, and of others which have either not been decomposed, or are not capable of re-uniting in the peculiar proportions mentioned. Dr. Hunter proved by experiment, that the food is *animalized* in the stomach. Some bread, which had been allowed to remain in the stomach of a dog for some hours, did not pass into the acetous fermentation, but became *putrid*. This peculiar change belongs to animal matter alone; and if that important change by which vegetable becomes animal matter, takes place in the stomach, any secondary process appears to be unnecessary; and therefore I conceive that the *chyle* is formed by the action of that organ."

What is the character of the above mentioned "several experimentalists," who could not discover any hour-glass contraction of the stomach, we cannot venture to conjecture; but we can assert that we have repeatedly seen that phenomenon exactly as it has been described by Sir E. Home, although we do not pretend to decide that it must always occur during the process of digestion. Of the truth of the facts quoted from the two Hunter's, we are not disposed to doubt. But they appear to us to be susceptible of a different explanation from that which the author has drawn from them. The kind of food with which the dogs were fed in Mr. John Hunter's experiments, is not specified; and the white fluid, by which his own curiosity does not appear to have been much excited, may have arisen from an admixture of milk with the food. No other physiologist has ever detected chyle in the stomach; and the probability is, that Mr. Hunter himself was not perfect-

ly satisfied with the opinion which our author advances in relation to this subject. That most careful observer would never have stated the circumstance with so few accompanying remarks, if he had intended to draw any positive conclusions respecting it. The results of Dr. William Hunter's observations upon the putrefactive disposition of the chyme, are also liable to equally strong objections. He allowed some bread to remain eight hours in the stomach of a dog, after which the animal juices which it had absorbed, putrified on exposure to the air. The vegetable matter might still have remained only partly changed, and its acetous fermentation have been disguised by the offensive products of the putrefaction of the gastric secretions. At all events, it is certain that the acetous fermentation frequently takes place in healthy stomachs; and it is now equally well known, that the sensible qualities of food are not always destroyed by the full action of the digestive liquor. Magendie declares that "there are as many kinds of chyme as there are sorts of aliments," and this opinion has been confirmed by numerous observations in man and in animals. We cannot agree with our author, therefore, in his opinion that the formation of chyle is completed in the stomach, and that the bile and pancreatic juice are of no other service than to stimulate the intestines to their natural peristaltic contractions.

In his chapter on absorption, he devotes three pages to the serious refutation of an idea, which we do not know that any body was ever silly enough to entertain,—that the oxygen of respiration gets admission into the blood through the medium of the absorbents. After discussing this point at some length, he very cunningly concludes, that as

"The lymphatics pour their contents into the venous blood *before it arrives at the lungs*, that change, therefore, in the appearance of the blood, which we attribute to the absorption of oxygen, ought, consistently with such an idea, to take place before, and not after, its passage through that organ; but the fact we know to be otherwise; and therefore we presume that the absorbents are not at all concerned in that action by which, in the process of respiration, the oxygen of the air unites with the blood."

But we will not criticise the laughable gravity of this kind of reasoning, lest we should be suspected of entertaining some jealousy at his dignifying himself with our prerogative right to the plural number.

A little further on he solves the knotty question, respecting cutaneous absorption, by the following right handy conclusion. "If the cutaneous absorbents do not act upon the surrounding atmosphere, there is no reason to suppose that, in a state of health, they act upon any thing which is exterior to the body itself." Respecting the origin and use of the lymphatics, he despatches, at one blow, the doctrine of Humpage, the opinion of Mascagni, and the researches of Brodie and Magendie. They are "generally considered to be erroneous," —they have become "almost obliterated by time," and he thinks it "wholly unnecessary to revive opinions which have died a natural death!"—But who, let us ask, has refuted the assertion of Mascagni, in regard to the connexion between the arteries and lymphatics; or who has overthrown the conclusions of the French physiologist, concerning venous absorption? It surely is idle to declaim without proof; and it is equally unreasonable to reject positive evidence because it happens to be opposed by more fashionable doctrines.

Under the head of Respiration, after having quoted from Dr. Henry an account of the chemical changes in the composition of the atmospheric air, he observes that,

"The process of digestion is here satisfactorily described. The principles of the air are *disunited;* the oxygen, that is, the nutritious part of the air, is lost in inspiration; and nitrogen, its excrementitious part, is separated from it, and eliminated from the system by expiration. I shall have occasion, in a future part of these observations, to advert to the generally received opinion, that the oxygen lost is contained in the carbonic acid expired; but I cannot enter upon this part of my subject at present, because it is intimately connected with the opinions I entertain of living action in general, which opinions have not yet been presented to the consideration of my reader. I shall, therefore, for the present, assume the fact that the oxygen contained in the carbonic acid expired, is *not* the individual oxygen which was conveyed to the lungs in the previous inspiration, that is, the inspiration immediately antecedent to its evolution from the sys-

tem in the act of expiration. That the oxygen of the air *unites with the blood* in its passage through the lungs, appears to be admitted by the concurrent testimony of the most eminent physiologists."

To prove that oxygen is actually introduced into the circulation, he quotes the *opinions* of Saumarez, Paris, and Cuvier; and it is really amusing to see in what manner he occasionally contrives to get the advantage of this kind of assistance. For instance, Dr. Paris, in describing Bichat's doctrine of the source of muscular irritability, happens to make use of the words, "*duly oxygenized blood.*" Our author immediately seizes hold of the sentence which contains this fortunate expression, and brings it forward as a proof in favour of his theory. But he stumbles at the fact that "we are unable, by the most delicate tests, to show that arterial contains an *iota* more of oxygen than venous blood." The following quotation will show how he endeavours to get over this difficulty, at the same time that it will elucidate his besetting idea of ærial digestion.

"Had it been proved that arterial contained more oxygen than venous blood, no difference of opinion could now exist as to the fact whether the oxygen of the air does or does not unite with the blood in its passage through the respiratory organs. We are, therefore, under the necessity of admitting, that according to the views of the most eminent physiologists, with the exception of a few individuals, the oxygen of the air does unite with the blood, but cannot subsequently be discovered in it. How can we reconcile this fact with the phenomenon observed, without allowing that the oxygen must have entered into a new combination, by which its original properties are lost. This product of respiration, without, at present, adverting to the manner in which it has been formed, may be, and has been, denominated the *arterial influence.* In chyle, the product of digestion, we cannot discover the principles which previously existed in our food; in the arterial influence, the product of respiration, we cannot discover the principles which pre-existed in the air. It appears, then, that the component parts of the air are first disunited; the oxygen, the *nutritious part*, combines with *some other substance*, in those peculiar proportions which constitute the arterial influence; that nitrogen, the excrementitious part, is separated from the air for the purpose of being eliminated from the system in the act of expiration; and that the product of these combined actions, is a substance essentially different from the atmosphere to which its origin must be attributed."

When we first encountered his idea of a "some other substance," which, by uniting with the oxygen of the air, constitutes the arterial influence, we confess that we were put to our wit's end to discover its meaning. In the concluding chapter of the book, however, we came in full contact with the very thing itself, in the shape of electricity, which he seems to imagine is given out from the lungs to the atmospheric air. The same agent, he believes, is given out from the mucous membrane of the stomach, to the various injesta, by means of which their nutritive particles are converted into sound and well digested chyle. Now, seriously, this is carrying the point a little too far. We ourselves have always experienced a lurking disposition to lay upon galvanism a sufficiently hard task to perform; but when it is told to us that, in addition to the excitation of secretion, of nervous sensibility, and of muscular contraction, the same influence digests the air, the food, the blood, the lymphatic fluid, and almost every thing else in and about the body, we must confess that our credulity is taxed to the very utmost point of endurance.

In the chapter on excretion, we meet with a well attested, and ingeniously supported fact, which is worth a whole multitude of mere opinions, such as our author has quoted to prove his doctrine of respiration. It is, that carbonic acid gas is given out from the blood through the cutaneous exhalents. We are surprised that Dr. Shute did not bring this forward in his argument respecting the oxygenization of the blood, inasmuch as we conceive that it completely establishes that theory. The carbonic acid gas of expiration can originate from only two sources,—either from a direct combination of the atmospheric oxygen, with carbon excreted from the venous blood, or from a direct exhalation of carbonic acid, by the pulmonary capillaries. Now, if this gas is actually exhaled from the whole external surface, there can be no reasonable doubt that it is exhaled also from the lungs. That this is the case, we think the following circumstances, which are detailed at full length by our author, will abundantly prove: Count de Milly once

observed, while laying in a bath, "a prodigious quantity of small silver-coloured air bubbles, resembling what are called the seeds of pearls, on the surface of his body" These he collected in a common decanter, and submitted to Lavoisier, who ascertained that they were composed of pure fixed air. Dr. Priestley, and others, afterwards attempted to explain this phenomenon, by the supposition that the water must have contained oxygen gas, which united with carbon excreted from the skin, and thus became converted into fixed air. Spallanzani and Abernethy, however, have refuted this supposition by various experiments, in which carbonic acid was produced from the surface, independent of the presence of oxygen in any shape. The results of Spallanzani's experiments are detailed in a letter to Senebier, introductory to his memoirs on respiration.

"He appears to have been completely satisfied that the air in contact with the skin, is changed exactly in the same manner as by respiration. When atmospherical air was employed, oxygen disappeared, and carbonic acid was produced; but he is of opinion, that the oxygen of the atmosphere had no share in the production of the carbonic acid gas, because it was equally great when the animal was confined in gases containing *no oxygen;* and he therefore concludes that the carbonic acid gas was exhaled, and the oxygen absorbed."

Mr. Abernethy's experiments, as described in the Ed. Med. and Surg. Journal, for 1805, and quoted from that work by our author, were performed by introducing his hand and wrist under a glass jar, inverted in the mercurial trough.

1st—The jar being filled with quicksilver, he collected, in the space of sixteen hours, one-half ounce measure of air, which had escaped in small bubbles, from every point of the surface of the immersed hand. Two-thirds of this gas were absorbed by lime-water; the remainder suffered no diminution by the mixture of nitrous gas. *2d*—The experiment being repeated under water, a small quantity of azotic gas only was obtained, the carbonic acid having been absorbed in the water. *3d*—The jar contained seven ounces of azotic gas. At the

end of two hours, rather more than one ounce of carbonic acid gas was produced. Similar experiments were made with hydrogenous, nitrous, and oxygenous gases; and in every case the quantity of carbonic acid obtained, was nearly the same. "Experiment shows, then," says Mr. Abernethy, "that carbonic acid gas is *perspired* from the vessels; for into whatever air the hand be immersed, the quantity of carbonic acid given out will be nearly the same."

The venous blood is commonly supposed to owe its dark colour to the presence of an excess of carbon, which is excreted in the lungs for the purpose of uniting with the oxygen of the atmosphere. The arterial blood must, therefore, be deprived of this superabundant combustible matter, in order to acquire its florid hue and peculiar influence. Professor Abilgaard, of Sweden, however, completely refuted this idea by some careful experiments, which appeared to prove the very opposite state of things in regard to the two kinds of blood. Instead of detailing this important fact under the head of respiration, our author brings it into his chapter on *living action.*

It has been commonly supposed to be an established point, that the oxygen inhaled, corresponds to the carbonic acid exhaled; or, in other words, that the whole of the oxygen which disappears, is contained in the carbonic acid generated in respiration. From several quotations in relation to this subject, our author concludes, and apparently with great justice, that the conversion of 1.2 cubic inches of oxygen into the same quantity of carbonic acid gas takes place in every inspiration. He goes on, however, to observe,

"But if we pursue this subject further, we shall find that this proportion between the oxygen lost, and the carbonic acid generated, is not maintained. Sir H. Davy found that hydrogen gas could be respired with impunity, and that, in its passage through the lungs, it experienced no change. Here, then, was an easy mode of inspiring the residual instead of the atmospherical air, and what was the result? The bulk of this residual air reduced to the temperature of 55°, he estimates at 32 cubic inches, being composed of nitrogen 23, oxygen 4.9, carbonic acid 4.1, which is 15.3 per

cent. of oxygen, and 12.8 per cent. of carbonic acid. Now, in proportion as the carbonic acid increased from 8 to 12.8 per cent., the oxygen should have diminished from 13 to 8.2; but the carbonic acid has increased 4.8 per cent., and the oxygen, instead of being diminished 4.8 per cent., is increased 2.3, so that there is a surplus of 5 per cent. of oxygen."*

Mr. Ellis proved by his experiments, also, that the volume of oxygen which is lost, exceeds that of the carbonic acid thrown out by expiration; and, indeed, this excess was so apparent to Lavoisier, as to lead him to conclude that it was employed to form a part of the vapour of exhalation, by uniting with the hydrogen of the blood. In addition to these circumstances, our author brings forward the fact, that carbonic acid is generated by seeds, without any supply of oxygen, to show that it must be given out from the system, in the form of an excretion. Although we are not disposed to detract any thing from the force of such arguments, we cannot help expressing our surprise that Dr. Shute should have overlooked the circumstance that carbonic acid is actually contained in the blood. Sir Everard Home concluded from his experiments, that twelve cubic inches of this gas are contained in every eight ounces of blood; and Dr. Scudamore, although he could not detect so large a proportion, supposes that the fibrin is rendered soluble in the fluid parts of the blood, by the presence of the same agent. We regard this fact as conclusive upon the point under consideration, and shall not, therefore, attempt to analyze the numerous other circumstances discussed under this head by our author.

Upon the subject of the circulation, the author is not so bold as from the perusal of his introductory chapter we were led to anticipate. When we read of the process of circulation being a part of the grand digestive function, we fully expected to meet with something new and extraordinary in this chapter.

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* To understand the above, it should be recollected that Allen and Pepys have ascertained that 100 parts of expired air consist of nitrogen, 79; oxygen, 13; carbonic acid, 8.

The late experiments of Prevost and Dumas have proved that after the removal of the kidnies, the blood becomes impregnated with the peculiar qualities of urine. In obstructions and derangements of the structure of the liver, also, we frequently meet with the most undeniable symptoms of a bilious contamination of the fluids. These circumstances certainly render it probable, that the peculiar proximate principles of the various secretions, are elaborated in the blood as it circulates towards the different glands. Our author, however, does not take advantage of these circumstances to bolster up his favourite doctrine, but proceeds as follows:

"The blood is a fluid *sui generis*, that is, different from every other with which we are acquainted, consisting of certain substances derived through the medium of the absorbent system from every part of the animal frame, and through the medium of the respiratory organs from the surrounding atmosphere. All attempts to ascertain the nature and constitution of this fluid have hitherto proved fruitless. We are absolutely unacquainted with the nature and peculiar properties of those substances from whence it is immediately formed: we know, for instance, that it consists of a certain fluid derived from the intestinal canal, through the medium of the lacteal absorbents, of another fluid derived from every part of the body, through the medium of the lymphatic system, and of a certain substance derived through the medium of the respiratory organs from the surrounding atmosphere. We know that the first, denominated chyle, and that the last, denominated oxygen, are destined to be conveyed to the several parts of the system, for the purpose of being incorporated with it; and we have the strongest reasons for believing that the other substance, denominated lymph, having already accomplished its destined function in the system, is intended to be conveyed to different organs of the body, for the purpose of being eliminated from it; but we are absolutely unacquainted with the nature of chyle, oxygen, or lymph. This observation, as applied to oxygen may at first sight appear untenable; but we have every reason to suppose that oxygen does not unite with the blood so as to form a component part of that heterogeneous fluid, until it has previously united with some other substance, by which its characteristic properties are lost."

From this extract it will be seen that our author is very careful in discriminating between the recrementitious and excrementitious parts of the blood. Indeed, he goes so far, in one place, as to assert that the oxygen which is given out

from the lungs, in union with the carbon of the carbonic acid, is derived from the solid parts of the frame, in which it must have existed a sufficient length of time to have its active properties exhausted by constant use. We must confess, however, that this idea appears to us to be an over scrupulous piece of refinement, as far as regards the accuracy of division between the two kinds of particles. The old materials may, for aught we know, acquire new properties from being commingled with the circulating mass, so as to become again fitted for the purposes of nutrition. But even on the supposition that all those particles which have once been employed in the animal organization, are totally unfit for a renewal of their combination with the same parts, it does not involve the necessity of their being disqualified for a union with parts possessing a differently organized structure. To attribute to the capillaries a faculty of rejecting all the old particles of oxygen, and other elementary ingredients of the system, in favour of those recently introduced into the circulation, would require a greater degree of faith in the doctrines of Stahl, than we are willing to entertain. Our author, however, perseveres so diligently in hammering out this one of his most favourite opinions, that he occupies no inconsiderable proportion of several chapters with its details.

Mr. Hunter's doctrine of the vitality of the blood, is opposed by our author in round terms. "How can we consider," says he, "that to be a living action, which never takes place in a living system, except as the consequence of disease?" In thus alluding to the coagulation of the blood, which Mr. Hunter supposed to proceed from its vitality, he seems to think that he has hit the very nail on the head. He goes on, a little further, to sneer at the inconsistency of attributing life to the blood, for the purpose of producing that very same coagulation which the life of the vessels alone can prevent. This kind of reasoning, however, falls very far short of that which our readers will no doubt recollect to have been impressed with in our analysis of Dr. Scudamore's work upon the blood.

In the chapter on secretion, a very succinct and interesting view is given of the mechanical, chemical, and electrical theories of the causes by which that mysterious process is carried on in the animal economy. Of the chemical we would remark, however, that it should be comprehended under the electrical theory, inasmuch, as all changes in composition are now supposed by the chemists to be effected by the galvanic influence. In relation to the electrical theory, we extract the following passages, both because they are in themselves interesting, and because they will serve to show the state of medical opinion in Europe upon this subject:

"It must be admitted that the discoveries of Galvani, Volta, and others, have not produced that sudden revolution in the science of medicine, which was anticipated by these celebrated philosophers, when the phenomena of galvanic electricity were first observed; but it appears to me, that the progress towards that revolution is clearly discernible, is every day more apparent, and I am sanguine enough to expect that it will ultimately be complete. A considerable period of time has now elapsed, since the early experimentalists, yielding to that enthusiastic spirit of discovery which too often dazzles but to deceive, boldly asserted that the galvanic influence was life; but man, though often led away by his feelings, is a rational animal, and he soon discovered that the galvanic fluid was *not* life. We all know that if a pendulum be impelled to a certain distance on one side of its centre of motion, it will, by its natural reaction, return to the same distance on the other; so has it been with the galvanic philosophers; the galvanic fluid was not *life*, and therefore it was *nothing*; galvanism did not *regulate* the actions of the system, and therefore it had *nothing* to do with them. But the vibrations of the pendulum are contracting upon their centre, and we are gradually withdrawing ourselves from the extremes of enthusiasm on the one hand, and of despair on the other. We now admit that galvanism is neither life, nor the comptroller of living action, but at the same time feel convinced, that it has an intimate connexion with both."

"Mr. Abernethy says,—'I consider it already *demonstrated* that they are electrical actions which cause the various combinations and decompositions so constantly occurring in all parts of the body.'"

"Sir Everard Home says,—'By these experiments it is ascertained, that a low negative power of electricity separates from the serum of the blood an alkaline solution of albumen; that a low positive power separates albumen with acid and the salts of the blood, that with one degree of power albumen is separated in a solid form, with a less degree it is separated in a fluid form.'"

"Such decomposition of the blood by electricity, may be as near an approach to secretion, as could be expected to be produced by the artificial means at present in our power. The discovery of an electrical power which can separate albumen from the blood in a fluid state, may explain the mode in which different animal solids and fluids may be produced: since, according to Mr. Hatchett's experiments, albumen is the principal material of which animal bodies are composed."

Dr. Young has attempted to inquire a little more minutely into the nature of secretion, with what success he leaves his readers to determine for themselves.

"We may imagine, that at the subdivision of a minute artery, a nervous filament pierces it on one side, and affords a pole positively electrical, and another opposite filament, a negative pole; the particles of oxygen and nitrogen contained in the blood, being most attracted by the positive point, tend towards the branch which is nearest to it, while those of the hydrogen and carbon take the opposite channel; and that both these portions may again be subdivided, if it be required, and the fluid thus *analysed* may be *re-combined* with new forms, by the re-union of a certain number of each of the kinds of minute ramifications. In some cases, the apparatus may be somewhat more simple than this; in others, perhaps, much more complicated; but we cannot expect to trace the processes of nature through every step; we only inquire into the general direction of the path she pursues, as much in order to avoid being led away by false opinions, as for the sake of any direct advantage that can be gained from our partial views of the true state of the operation."

Our author attributes some degree of weight to the suggestion of Dr. Ontyd,—that electricity unites with the living power, and becomes divested of some of its natural properties, by means of which union it becomes *animal electricity*. He concludes that all the theories of secretion agree in the point, that the principles of the blood must be "disunited, and recombined in a new manner, and in different proportions." This circumstance he takes good care to dispose of in such a way as to bear upon his favourite digestive notion. He thinks that secretion is the same thing as digestion, and is performed by the very same agent; the only difference consisting in the circumstance that food is concerned in one case, and blood in the other.

In the chapters on excretion and nutrition, we can discover nothing peculiar, except that our author labours hard to prove that the former process is wholly devoted to the removal of the effete particles from the system; while the latter is equally confined to the appropriation of those newly introduced to the several parts of the whole animal structure. Upon the uncertainty of our knowledge upon this subject, we have already remarked, and we shall, therefore, avoid further comment upon it on the present occasion.

In the chapter on electricity, some interesting facts are detailed respecting the natural accumulation of that fluid in living bodies. In cold climates, there is always a disposition to this phenomenon under certain circumstances; but such cases as our author has extracted from various publications, are not witnessed by many observers. The former grand duke of Russia could give sparks from his hands and face; and Count Orloff could almost light up a room by drawing a comb through his hair. On the top of Mont Blanc, Messrs. Pictet, Jalabert, Saussure, and servants, could hear the electric fluid pass off from their buttons with a buzzing noise; while their fingers at the same time smarted with the streams which were passing off from them. Messrs. Tupper, Lanfier, and guide, on descending Mount Ætna, found that musical sounds were produced by the rapid passage of electricity from their hands to their heads, &c. which could be heard to the distance of forty feet. This last mentioned fact is particularly interesting, inasmuch as it proves that there may be different quantities of electricity existing in different parts of the body at the same period of time. The phenomena presented by the torpedo and gymnotus electricus, however, prove the same thing much more conclusively. The experiments of Humboldt, and Gay Lussac, have proved that animal electricity is very different, in some particulars, from common electricity. They ascertained that the torpedo might be touched with impunity, by means of a key, or any other metallic body; and that an electrometer of the most sensible

kind was not at all affected by the presence of the fish. Mr. Flagg also found "that the animal might be touched with a metallic rod enveloped in a silk handkerchief, with impunity," and many others have observed, that the shock could not be transmitted through two or more persons, "unless they wetted their hands." Our author explains these facts by supposing that the electric fluid "undergoes *certain changes*, and is modified by the actions of the living system, by which it acquires properties peculiar to itself, and becomes in reality, not the electricity of the atmosphere, but *animal* electricity."

It is a singular fact that, after the torpedo has lost its electrical powers under water, they can be restored again by exposing it to the air. Mr. Flagg ascertained that this fish "raises its head every five minutes above the water, as if to respire." These circumstances appear to prove that animal electricity must be derived from the air in respiration.

After being stunned by a blow on the head, the torpedo loses its power of communicating a shock. Our author draws from this fact the inference that the shock does not arise from the general accumulation of electricity in the system, but from the influence of the will. He presumes that it is capable of being concentrated by the will of the animal, and that this power is intimately connected with its nervous system. He quotes the dissections of John Hunter, which detected an "immense size of the nerves that go to the electrical organs." Sir Everard Home has proved that "in these fish, the nerves connected with the electrical organ exceed those that go to all other parts of the fish, in the proportion of *twenty to one.*"

Dr. Shute thinks that the free electricity with which a man becomes charged by standing near a highly electrified body, has very little influence upon the system. He says that this fluid "thus *undigested*," affects animal matter no more than a living seed which passes unchanged through the bowels. The body must, therefore, be constantly charged with two

kinds, or modifications, of electricity,—the one free, the other combined with "some other substance," in such a proportion as to alter its ordinary properties. This leads us to the consideration of the very sum and essence of all our author's reasoning,—the nature of the *nervous influence.* It is contained in the middle of his book, to which the fore-part is merely introductory, and from which the after-part is drawn out like the application of an interminable sermon.

The common opinion respecting the nature of the nervous influence appears to be, that it is either pure electricity, or some modification of that fluid, by means of which the various organs of the system are excited into action. Irritability, or the power of being excited, is supposed, on the contrary, to depend on the presence of the oxygen, which distinguishes arterial blood. The source of both these agents is attributed to respiration, by which they are imagined to be introduced into the blood for the purpose of being distributed to all parts of the body. According to our author, however, the nervous and the arterial influences are both constituted by the same agent; and this he believes to be a combination of certain proportions of electricity and oxygen.

"Living action may be dependent upon oxygen *and* electricity, but not upon either exclusively. Millions of animated beings have been sacrificed upon the altar of experimental inquiry, and what is the result? We cannot at this moment determine, whether the action of the heart be dependent upon the arterial or the nervous influence, or whether it may not be dependent upon both. Is not this, at least, a presumptive proof that the arterial and nervous influence may be the same things? In support of the opinion that the nervous influence may be a peculiar combination of oxygen and electricity, I may adduce the example of galvanism."

To support this analogy between galvanism and the nervous influence, he resorts to the well known fact, that after an organ has lost its power of acting, in consequence of a division of its nerve, if the galvanic influence be applied to the inferior portion of the divided nerve, the organ *recovers* its power of being excited into action, by an appropriate stimu-

lus. After citing Mr. Philip's experiments on the stomach, he proceeds to say,

"If these experiments do not point out the identity of, they at least establish the striking analogy between the galvanic and the nervous influence. If, therefore, we have reason to suppose that the galvanic influence may be a peculiar combination of oxygen with electricity, we have the strongest possible analogical proof that the nervous influence is also a combination of these two substances. The nervous and the galvanic influence resemble each other in the effects they produce, and, therefore, we have the strongest possible reason for supposing that they resemble each other in the nature of the principles which enter into their composition."

To prove that galvanism is a compound of electricity and oxygen, he draws all sorts of logical inferences from Mr. Cruickshank's theory respecting *oxygenated* and *de-oxygenated electricity*. The only substantial argument, however, which he has advanced in relation to this point, is, that the presence of oxygen is always necessary to the development of galvanism. So invariably, indeed, is this the case, that many chemists have been driven to the conclusion, that the electrical phenomena which accompany various chemical decompositions, are produced by some change in the state of the oxygen, that exists in all substances capable of undergoing such decomposition. Now, as the presence of oxygen is equally necessary to the production of arterial blood, our author thinks himself entitled to draw the conclusion, that galvanism and the arterial influence are the same things. In confirmation of this opinion, he has brought forward a very interesting fact, which is the only one, as far as we have been able to discover in the whole book, that he has taken the trouble to put to the test of experiment. It is that galvanism converts venous into arterial blood.

"The analogy is still further pointed out by experiment. The peculiar property of the arterial influence, is to change the dark hue of venous, into the scarlet colour of arterial blood. The galvanic influence will also produce this change in the colour of venous blood. If the wires attached to each end of the galvanic trough, be brought into contact, or into a certain state of approximation, under the surface of venous blood, bubbles of a

vivid scarlet hue immediately rise to the surface. As often as the experiment is repeated, the same phenomenon may be observed. If a small portion of the dark coagulum of venous blood be allowed to attach itself to each wire, that of the positive wire will assume a brighter appearance, and that of the negative wire will be little, if at all, altered in its colour; but as soon as the two wires are brought into contact, the coagulum attached to each will assume a bright scarlet colour."

Mr. Wilkinson and Mr. Brande have made the same observations. The experiments of the former gentleman are detailed in his essay on galvanism, those of Mr. Brande were published in the Philosophical Transactions for 1809.

The next point which our author attempts to prove, is, that the arterial and nervous influences are the same things. The nervous influence he denominates "the secondary source of power;" and he supposes it to be abstracted from the arteries, and accumulated in the nerves, not for the purpose of exciting, but for that of supporting the vital properties. He arranges the points of analogy between these two influences under the following heads:

"Oxygen is necessary to the excitement of both. They both influence living action in the same manner.

"There is reason to suppose that they change the colour of the blood in the same manner.

"There is reason to suppose that they both contain electricity.

"There are no facts which point out any dissimilarity between the two, considered either as to the nature of their constituent principles, or as to their agency in the animal system.

"And lastly, the phenomena of living action seem to indicate the necessity of this secondary source of power. The effect being the same, we have a right to presume that the course is also the same."

Upon the second of these analogies, that they change the colour of the blood in the same manner, he throws out some curious hints. He thinks that the sudden change from dark to red blood, which occasionally takes place in venesection, results from an immediate communication of the galvanic influence of the nerves to the venous blood, just before it escapes from the orifice. He supposes, also, that in inflammation all the blood, which returns from the inflamed part is

reddened or arterialized in the same way. He demonstrates the necessity of a secondary source of power in the nerves, by a reference to the state of the heart while it is recovering from syncope, or suspended animation.

"Supposing the action of the heart to be suspended, or even diminished, how could its power be restored under any circumstances? The power of the heart, according to this theory, (the theory of Bichat) is wholly dependent upon itself; that is, upon the supply of the arterial influence through the medium of the coronary arteries, which supply pre-supposes an action of the heart itself. Thus the heart must act in order to acquire the power of doing so. Or supposing its action to be diminished, a weak action of the heart implies a diminished supply of the arterial influence. There can be no increased supply of the arterial influence without an increased action of the heart; so that the heart must act more powerfully, in order to acquire the power of doing so. But this increased action of the heart is a debilitating process; is itself a source of exhaustion; so that the heart must exhaust itself still further, as the only means of acquiring power. Is it not manifest that there must be some *other* source of power? It has been supposed that under these circumstances the nervous influence may act as a stimulus to the heart; but increased action, the effect of a stimulus, is itself a source of exhaustion: so that the heart must still exhaust itself as the only means of acquiring power. What is the simple explanation of the fact? The nervous influence is a secondary source of power, is a secondary source of the arterial influence. This unites with the heart independently of any increased action of the heart itself, and thus imparts power without the necessity of any debilitating action of that organ. In cases of suspended animation, we have resource to the artificial supply of oxygen through the medium of the respiratory organs: in fainting we apply a stimulus to the nervous system, and we succeed in restoring the action of the heart by these apparently different remedies. The heart itself, in both these cases, is in the same state. Its irritability must be restored. How do these different remedies produce the same effect? If the nervous and arterial influences be the same things, the question is easily answered."

It is hardly necessary to proceed further with our author's views. He has introduced galvanism fairly into the system; and with such an all-pervading agent an ingenious theorist can do any thing within the bounds of imagination, or of possibility.

M.

ART. V.—*A Compendious System of Midwifery, chiefly designed to facilitate the inquiries of those who may be pursuing this branch of Study, &c.* By W. P. DEWEES, M. D., Lecturer of Midwifery, &c. Philadelphia. H. C. Carey & I. Lea, 1824. Octavo, pp. 602.

AMONG the multitude of new medical works that are continually issuing from the press, we find but few, comparatively speaking, which deserve to be regarded as real accessions to our literature. The mass of clumsy compilation, of senseless theorising, and imperfect experience, which is constantly obtruded on the attention of the profession, is such as to alarm even omnivorous dullness itself. Now and then, however, we are presented with works of a different and more useful character,—with practical books, which give the result of a long course of enlightened observation and experience,—or with well directed and rational disquisitions on subjects of a theoretical nature. It is among this latter, and more useful class of publications, that we would rank the work whose title is at the head of this article. It is by no means a mere compilation of the opinions and experience of other writers. Its merits are of a higher order, being, in a great measure, drawn from the author's own extensive experience and reflections upon the subjects which it treats. Much, however, as we admire the many sound practical lessons contained in this book, we cannot help remarking, that in point of language, it falls short of what we had reason to expect from the author. It is undoubtedly true, that the mere literary merit of a practical book, is but of secondary consideration,—and that such a book is valuable in proportion to the value of its materials. We are not, therefore, disposed to dwell with a censorious spirit on the style of this work, which, though not to our taste, is sufficiently perspicuous and lively, to enable any one to read it without fatigue or irksomeness from this source. Whatever defect we may suppose it to have in this respect, we feel assured, that it is calculated by its intrinsic worth to reflect much credit on its author; and we take

pleasure in thus offering our testimony in behalf of its excellence. We proceed to a short notice of some of the more interesting portions of the work. The first six chapters are devoted to the consideration,—1*st*, *Of the Pelvis;*—2*d*, *Of the Child's Head;*—3*d*, *Of the Genital Organs;*—4*th*, *Of Menses;*—5*th*, *Of Conception*, and 6*th*, *Of the Action of the Uterus.*—In the 7*th* chapter, the author treats of the displacements of the uterus. *Retroversion* of this organ, according to Denman and Merriman, is not an accident of serious moment. Hunter, Baudaloque, Burns, and other writers, on the contrary, look upon it in a very different light. Dr. Dewees, also, is directly opposed to the two former gentlemen on this point, and he has given, we think, a satisfactory refutation of their opinions.

The symptoms of this complaint are more or less intense, according to the size which the uterus may have acquired, or according to the suddenness or slowness with which the retroversion is effected. "When suddenly induced, the symptoms are usually violent and alarming,—such as an immediate interruption to the flow of urine, or the passage of the fæces; alternate pains, accompanied by great forcing or bearing down; a disposition to fainting, &c. When considerable time is spent in completing this displacement, the evils arising from it are less urgent and severe. But in both cases, if the uterus be not restored, the symptoms will increase in intensity; instead of merely a difficulty and frequent inclination to make water, there will be a total suppression of it, accompanied by a painfully intense desire to do so, for the fœtus will go on to increase in size, and the uterus to develop itself, giving additional pressure to the parts with which it is in contact."

Dr. Denman thinks, that retroversion of the uterus may, in every instance, be referred to a distended bladder for its cause, and "that suppression of urine is absolute only before, or during the act of retroversion," and must therefore be regarded as the cause, and not the consequence, of the com-

plaint. Dr. Dewees objects to this opinion, because: "1*st*, we are certain that it has been suddenly produced by violence, and without the intervention of a suppression of urine. 2*d*, because Baudaloque demonstrated, before his class, a slow retroversion of the uterus, which lasted three or four weeks before it was complete; in this case there is no mention of any difficulty in making water."

According to Denman, every suppression of urine, occuring about the third or fourth month of pregnancy, and "continuing a *certain* length of time," with a certain degree of distention of the bladder, should be regarded as a case of retroverted uterus. Dr. Denman, forming his diagnosis of this complaint upon such grounds, we need not wonder, observes Dr. Dewees, that he considers this as a trifling disease. For "should every suppression of urine in a pregnant woman really constitute retroversion, as is declared by Dr. Denman, we can readily account for his indifference to its consequences, and his trusting its cure to nature, or the occasional drawing off the water by the catheter."

When retroversion of the uterus has taken place, we may generally succeed in restoring the fundus to its proper position, by the occasional employment of the catheter, and of mild laxatives or injections. If we fail in removing the complaint by these means, "we are then maturely to consider the propriety of mechanically replacing the fundus." There are three circumstances, says Dr. Dewees, which must be taken into view, in deciding upon this point. 1*st*, The period of gestation;—2*d*, The degree of development of the uterus;—and 3*d*, The nature or severity of the symptoms. The nearer the accident occurs to the fourth month of gestation, the greater will be the necessity for a prompt restoration of the retroverted fundus. As some uteri are much more developed at three months, than others are at four, the development of this organ must also be taken into consideration when deciding upon this point; for where the womb is more expanded than it commonly is in other cases at the same pe-

riod, there will of course be a greater necessity for prompt interference. Where the symptoms are exceedingly severe, and the water cannot be drawn off with the catheter, in consequence of the impossibility of passing it into this viscus, we must take care "not to temporize too long, lest the bladder become inflamed, gangrenous, or burst," all of which terminations have been witnessed.

"From this we conclude," says Dr. D., "that the uterus should in every instance be restored when practicable, at, or very little after the fourth month; if left longer than this, the risk of not succeeding is every day increased; and we are firmly of opinion, that nothing can justify a neglect to do so at this time, more especially when it proceeds from the vain hope, that nature will relieve herself at the full period of gestation."

That the bladder may be much and long distended, from retention of urine, without producing retroversion of the uterus, Dr. D. is quite certain from his own experience; and he is equally convinced, "that in retroversion, the mere removal of the urine will but rarely, nay, not once, perhaps, in ten times, *where the complaint is of long standing, or the pregnancy advanced beyond the third month,* be sufficient to ensure the spontaneous restoration of the fundus." It must nevertheless be borne in mind, that the urine is always to be drawn off, where practicable, as often "as the exigencies of the case may require." This is indispensable either for the spontaneous or artificial reposition of the uterus.

Dr. D. thinks that the risk of abortion is but trifling; and that the apprehension of this consequence should not deter us from attempting to restore the uterus to its proper position.

The directions given by our author, for the best mode of performing this operation, are precise and judicious. The forces which resist the restoration of the displaced fundus, are:—"1*st*, A distended bladder.—2*d*, An impacted rectum. 3*d*, The counteracting efforts of the woman herself.—4*th*, The too great bulk of the uterus." The first thing to be done, therefore, is to remove these obstacles as much as possible. To obviate the resistance offered by the involuntary efforts of the

woman to bear down, Dr. Dewees recommends bleeding "to, or near to, fainting." When bleeding is determined on, "we should be prepared before hand to take advantage of the deliquium, as its effects are but transitory." Every thing should therefore be prepared in such a manner as to enable the accoucheur to proceed to the operation without delay or inconvenience during the period of syncope.

When blood has been drawn until faintness is induced, and the woman is placed in a proper position,

"The hand, after being well lubricated, should be passed into the vagina, to a state of supination, the fingers retracted in such a manner as to form a straight line at their extremities; they must then be gently pressed against the base, as it were, of the tumour that is found within the vagina, so as to move it backwards and upwards along the hollow of the sacrum, until the mass shall reach above the projection of this bone; when thus far, the hand may be withdrawn, and a pessary be introduced of a proper size. The woman must remain quietly in bed for three or four days; the urine for this period should be drawn off as often as may be required, and the fæces evacuated by injections."

The author's observations "*on the obliquities of the uterus,*" are interesting, and deserve to be attentively read. When the fundus of the uterus continues to advance, "in the direction it received when passing through the superior strait," without being sufficiently resisted by the pressure of the abdominal parieties, it acquires what is termed the "anterior obliquity." The abdomen becomes pendulous, which is sometimes carried to a surprising extent, "especially in little women who are much upon their feet, and in those who have a deformity of pelvis." The inconveniences of this condition, in the latter period of gestation, are often very great; such as severe pain in the back, loins, and hips, with a distressing sensation of bearing down.

This anterior projection of the uterus is sometimes a source of considerable embarrassment to the midwife, as well as of distress to the patient. The os uteri, in cases of this kind, is often thrown so far out of the proper axis of the pelvis,—being turned back towards the projection of the sacrum, that

the head will descend into the pelvis, "covered by the stretched anterior portion of the uterus." It is therefore necessary to attend early to the reduction of the os uteri to its proper position. To do this, in cases where the mouth of the uterus cannot be reached in the ordinary way, the hand must be introduced, "so that its palm may be next to the distended uterus;" a finger is next to be hooked into the os uteri, which is then to be drawn "towards the centre of the interior strait. When it has followed so far, the hand may be gently withdrawn, but not the finger from the os uteri, and the uterus detained there until the proper direction of the forces, and the axis of the uterus, are made to correspond."

The distressing consequences which may result from the want of a proper knowledge of this variety of uterine obliquity, is strongly exemplified by the following relation of our author:

"Within our own knowledge, this case has been mistaken for an occlusion of the os uteri, and where, upon consultation, it was determined that the uterus should be cut, to make an artificial opening for the fœtus to pass through; they thought themselves justified in this opinion, first, by no os uteri being discoverable by the most diligent search for it; and secondly, by the head being about to engage under the arch of the pubes, covered by the womb. Accordingly, the labia were separated, and the uterine tumour brought into view; an incision was now made by a scalpel through the whole length of the exposed tumour down to the head of the child,—the liquor amnii was evacuated, and, in due course of time, the artificial opening was dilated sufficiently to give passage to the child; the woman recovered, and to the disgrace of the accoucheurs who attended her, was delivered, per vias naturales, of several children afterwards; a damning proof that the operation was most wantonly performed."

Dr. D. observes, that it is sometimes sufficient for the restoration of the fundus, even in cases of extreme obliquity, simply to place the woman upon the back; the pendulous abdomen being at the same time raised and supported by means of a towel or napkin properly adjusted.

In the eighth chapter, the author treats of the signs "which usually attend pregnancy." He warmly, and, we think, suc-

cessfully, combats Dr. Denman's opinion, that the continuance of the menstrual discharges insures an exemption from pregnancy. It appears to us extraordinary indeed, that any one who has had Dr. Denman's opportunities of observation, should advocate this opinion. Dr. Dewees avers, that he knows many women "who habitually menstruate during pregnancy until a certain period. Several of these menstruated until the second or third month, others longer, and two until the seventh month." Within this year, we have ourselves seen two instances in which the menses appeared regularly, and in their genuine character; in one of them twice, and in the other four times, after the period of conception. The latter female informed me that the same circumstance took place in her two former pregnancies. Other writers mention similar cases; Dr. Dewees refers to Heberden, Hosack, and Francis, for evidence on this point. When facts of this kind can be adduced, it is useless to reason either for or against the possibility of such an occurrence. Dr. Dewees mentions a case that came under his notice, in which menstruation went on regularly during pregnancy, but ceased when the woman was not in this state. He observes, that so far as he has been able to ascertain, Deventer is the only writer who has furnished a similar example. Froriep, a late German writer on midwifery, refers to a case of this kind, recorded in Stein's Annals on Midwifery, Vol. I. p. 166. In this instance, menstruation was perfect and regular throughout the whole period of gestation, but was always wholly absent when the woman was in an unimpregnated state.*

The heart-burn which sometimes attends the early period of pregnancy, may be often effectually relieved, says Dr. Dewees, by eating a few blanched almonds, or roasted groundnuts. "These should be taken from time to time, as the acidity may prevail. It is of much importance, in cases of

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* Handbuch der Geburtshuilfe, von Lud. F. V. Froriep. Weimar, 1822, page 178. See, also, Stein's Annals of Midwifery, Vol. I. page 166.

this kind, to guard against constipation of the bowels.—Dr. D. has occasionally found the use of ship-bread, or of biscuit made of unbolted flour, to answer very well in such cases. Where this is insufficient, he advises the use of the following pills, which, when "properly persevered in," are often of great advantage:

℞		
	G. Aloes, Soc.	℥ss
	Pulv. Rhæi	ʒj
	Ol. Caryoph.	gr. iv
	Sapo. Venet.	gr. viii
	Syr. Rhæi	g. s. ut fiant pil. xxx.

One of these may be taken every night.

For the leucorrhœal discharge, which is often found to accompany pregnancy, Dr. Dewees directs the parts to be washed three or four times a-day, with *luke-warm* water, and a weak solution of sugar of lead to be thrown into the vagina with a syringe. Before the injection is used, however, "the parts should be washed with a weak solution of fine soap in warm water," by throwing a few syringes full into the vagina. "We are convinced," says Dr. D., "that this last plan will afford relief when the non-observance of it might not be followed by the smallest benefit." Dr. D. prefers *luke-warm* water to cold; and in this he differs, as he himself observes, "from almost every other practitioner." From long experience, however, he is fully convinced of the superiority of the former.

Our author's remarks on the treatment of fluor albus, in females, not pregnant, are well worth attending to. He divides the idiopathic forms of this complaint into three stages, "each of which requires a little difference of management."

"In the first, or most simple form, the matter discharged is glairy and transparent, or resembling a thin starch; this very often accumulates, from its tenacity, in considerable quantity within the vagina, and is then suddenly discharged, either by its own weight, or by some sudden effort of the woman, especially upon stooping, or lifting a weight,—this never becomes acrid, unless there is the most reprehensible neglect of cleanliness, nor so far as we have observed, is the system implicated, though it may take place

in constitutionally plethoric women. We always commence with a direction that the parts are to be regularly washed with warm water, three or four times a day,—if the patient be plethoric, we cause her to be well purged; confine her to a milk and vegetable diet, and sometimes order her to be bled; when the pulse is sufficiently reduced by these means, or if the pulse be in a proper condition without these means, we then commence with the tincture of cantharides; of this we direct thirty drops every morning, noon, and evening, in a little sugar and water, increasing the dose every third day, five drops at a time, until strangury is produced. We always direct our patient to desist from the use of the tincture, so soon as she feels the approach of strangury, and not to resume it again until all uneasiness disappears. Should the complaint withstand the first strangury, we are not discouraged, but recommence the tincture, of the original dose of thirty drops, and increase it as before, until a difficulty in making water is again experienced,—it rarely, however, withstands the second irritation of the bladder."

As soon as the discharge becomes thinner and more abundant, he employs astringent injections. These, however, should never be used until this change in the discharge has taken place.* He prefers, in cases of this kind, the acetate of zinc, in the proportion of five or six grains to an ounce of water. The second stage, or variety of this complaint, is characterized by a discharge of a "white or yellowish purulent appearance." From want of proper attention to cleanliness, this discharge may become offensive and excoriating. This form of the complaint is "always accompanied with pain in the back, hips, and in the region of the pubis; the woman's complexion is generally sallow, and when the discharge is excessive, she becomes subject to a train of distressing nervous symptoms." The pulse is almost always found to be hard, wiry, and irritated. Brisk catharsis, a vegetable diet, and bleeding, should never be neglected in the commencement of the treatment. After the pulse has been brought to a proper state by these means, the author directs the employment of the tincture of cantharides, as stated above, with saturnine injections.

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* The tincture of cantharides employed by Dr. Dewees, is "just fifty per cent. stronger than the ordinary tincture of the shops."

In the third stage, all the symptoms of the second are much aggravated. The discharge has a green colour, and is often tinged with blood.

The same plan of treatment is required in this form of the complaint, as in the preceding one, with this difference only, that the antiphlogistic remedies in the commencement must be more actively employed, and the dose of the tincture of cantharides more slowly and cautiously increased.

The *pruritus*, or itching in the pudendum, to which females are subject, especially during pregnancy, is sometimes exceedingly distressing.

"The precise nature of this affection," says Dr. D. "so far as we know, has never been pointed out; and accident furnished us with an opportunity of detecting the real condition of the parts where this complaint was in full force. A lady, whose husband was more notorious for his gallantries, than his domestic virtues, was attacked in the incipient stage of pregnancy with an intolerable itching in the pudendum, and even within the os externum along the vagina. Suspecting she was infected by a venereal affection, we were sent for, and she giving such an account of her feelings as to make us think it might truly be the case, we proposed an examination of the parts, which was finally acceded to. Upon separating the labia, the whole face of the vulva, the os externum, and as far in the vagina as could be viewed, *was covered with an incrustation of apthæ.* We assured the patient her complaint was not as she had supposed, but one which we hoped we could quickly remove. We accordingly ordered a strong solution of borax in water, and requested her to wash herself four or five times a-day with it, as well as to throw some of it up the vagina at the same time; she did so, and was perfectly well in twenty-four hours."

Dr. Dewees considers borax as nearly a certain remedy in this complaint. In obstinate cases, bleeding and purging are important auxiliaries.

The chapter on *Deranged Menstruation*, contains some novel and very judicious observations, and is full of sound practical advice. Some of the author's remarks "on the tardy appearance of the menses," appear to us particularly important.

"There seem," says Dr. D., "to be four conditions of the female system, in which the menses are tardy in their appearance:—1*st.* Where there is little or no development of the

genital organs;—*2d*, where this development is interrupted by a chronic affection of some part;—*3d*, where the most perfect development has taken place, but they do not make their appearance."

The first condition of the system is known by the absence of those signs which characterise puberty. "The breasts are not swelled, but remain dormant, as it were; nor is hair always protruded on the pubis."

"In a girl thus circumstanced," says Dr. D. "who, otherwise than the mere absence of the menses, is in good health, it would be more than idle, it would be cruel and dishonest, merely because she had attained her fourteenth and fifteenth year, to subject her to medical rule, or goad her system by stimulating emmenagogues. In such a case, if the mother or friends are rational, and to be trusted, we may honestly give our opinion of the entire insufficiency of medicine to produce the desired end. We should explain, so far as we can, the nature of the functions of menstruation, and of the pre-requisites to this discharge, and attempt to produce on their minds the important conviction, that time, under proper circumstances, is all that is required to effect the anxiously hoped for change. Where the friends, however, are importunate, and will not yield to this salutary advice, we should use a temporising plan, and, by the administration of some entirely inert medicine, gain time, and save the poor child from permanent ill health, or an untimely grave."

We have not room to give a particular account of what our author says on "the suppression of the menses;"—"the immoderate flow of the menses,"—"dysmenorrhæa,"—and "the decline of the menses." We cannot, however, forego quoting his remarks on the employment of *aloes* in that species of uterine hæmorrhage, which sometimes occurs about the period of the cessation of the menses.

"Against the use of aloes," he observes, "there is much clamour; but we have some reason to believe, not with sufficient justice. We do not wish, by any means, to decide this point at this time, as our experience in its use is as yet rather limited. We have thought proper to direct attention to it, that we may be aided by the experience of others, in determining the powers of this medicine. But we will relate what we know upon the subject, and leave it to the further employment of this drug to either confirm or destroy our present favourable impression of it. A lady, aged forty-two

years, for whom we had prescribed almost all the known remedies for the hæmorrhage under consideration, with very little benefit, was told by some old women, that the *hiera picra* was a certain cure in her complaint; she mentioned this to us, and we very candidly stated our own and the general prejudices against the principal ingredient in this compound; but, at the same time observed, that as the old woman who had recommended it, cited the cases of two or three ladies who were known to her, it would be easy to make the inquiry, and if it was as she stated, it would be well, as every thing else had failed, to give it a trial. The medicine was warmly recommended by these ladies, and she proceeded to make use of the old woman's prescription, which was half an ounce of hiera picra to a pint of gin; of this a wine-glass full was directed at bed time;—it was taken, and the lady was completely intoxicated all night, and very sick in the morning. To avoid this sickening effect of the medicine, we prescribed it for her in the following manner:

"℞	Hiera Picra - - -	ʒjj
	Ol. Caryoph. - - -	gr. x
	Sapo. Venet. - -	gr. xjj
	Syr. Rhæi - - -	g. s. ut fiant pil. xxxx.

"One of these was directed every morning, noon, and evening, unless they should prove too purgative. She soon perceived a diminution of the discharge, and by the time she had finished the pills, it was so much reduced in quantity, as to give no further uneasiness. Two cases of a similar kind, but of less severity, were entirely relieved after the use of the same formula."

As a general remedy in cases of this kind, Dr. Dewees has found the extract of cicuta highly serviceable. It should be commenced with in small doses, and gradually increased until marks of "its influence, such as vertigo, head-ache, or sickness of the stomach, begin to show themselves." When these symptoms are observed, the medicine is continued without increasing the dose, until they go off, when it must be again increased," and so on, until the complaint has so far yielded, as to render its farther exhibition unnecessary, or until we are convinced that it will not succeed in arresting it. Dr. D. reprobates the employment of tonics in this complaint, as highly injurious in the majority of cases. "They are only admissible," he observes, "where there is nothing but debility to contend with."

Chapter XII. *Of Labour.*—The rigors which some females experience in the very commencement, or during the progress of labour, seem, says Dr. Dewees, to be connected "in some way or other, with the dilatation of the os uteri." When these shiverings, (which are never attended with the sensation of cold,) take place during the progress of labour, Dr. D. "has always felt assured that the mouth of the uterus was dilating, or dilated."

With regard to the manner in which the os uteri is dilated, during parturition, Dr. D. has offered an explanation, which is both ingenious and plausible. Want of room, however, obliges us to omit an account of the Dr's. peculiar views on this subject; we therefore refer the reader to the work itself, for a satisfactory explanation on this head. We will merely observe, that he does not consider this phenomenon as the consequence of mechanical force, exerted by the distended membranes, or any portion of the child's head, opening the mouth of the uterus by a wedge-like action.

In relation to the "*after pains,*" the author observes, "that by the old women, (and we may add, by many accoucheurs also) the after pains are considered useful, because they are almost always accompanied by the discharge of coagula, which they say must come away, and on this account oftentimes refuse to give any thing for their relief. But we have always regarded them as an evil of great magnitude; we always endeavour to arrest them as quickly as possible,—they interrupt sleep, which is at this time of special importance, and they also needlessly excruciate the body." In these sentiments we perfectly agree with Dr. Dewees. He generally prescribes camphor for the relief of these pains, according to the following form:

"℞	G. Camph. - - -	℥jj
	Spir. Vin. rect. - -	g. s. f. pulv. adde
	Pulv. G. Arab. - -	℥jjj
	Sacch. Alb. - - -	g. s.
	Aq. font. - - -	℥vi. M.

"A table-spoonful of this mixture is to be given every hour or two, until ease is obtained."

Dr. Dewees states that he has seen a few cases of a peculiar and very distressing character, which he has not found noticed in any of the books on midwifery. "It is a most severe and constant pain at the very extremity of the sacrum and coccyx; it begins the instant the child is born, and perseveres with most agonising severity, until its violence is overcome by the rapid and liberal use of camphor and opium. It is declared by the patient, to be infinitely more insupportable than any pains of labour, for it is never-ceasing." In one case Dr. D. was obliged to administer nearly *five hundred drops* of laudanum before relief was obtained.

This same lady was similarly affected in her second and third confinements. In the second, laudanum was given in large and repeated doses, "which *eventually* overcame the pain." In the third confinement, however, the sufferings of the patient were much more speedily relieved, by administering a table-spoonful of the following julap every fifteen minutes, by which the pain was entirely subdued in the course of an hour:

"℞	P. G. Camph. - - -	ʒjj
	P. G. Arab. - - -	ʒjjj
	Tinct. Opii acetat. - -	ʒjjss
	Ol. junip. - - - -	gr. xx
	Sacch. Alb. - - -	g. s.
	Aq. fontanæ - - -	ʒvi. M.

Another case which fell under the Dr's. notice, was treated with this julap, and the result was equally speedy and favourable.

Under the head of "*Exhaustion*," in his chapter on "*the causes of preternatural labours*," the author makes some remarks which appear to us worthy of much attention. The "*exhaustion*" of which he here speaks, "is not the mere loss of strength in the muscular system, but a state of inertia of the uterus itself." This state of inertia of the womb

may arise from different causes, and require different modes of treatment. It may be the consequence of an over distention of the uterus, from an excess of the liquor amnii. In cases of this kind, the pains return at irregular intervals, and are "confined to the uterine globe, which is excessively distended, with very little of that bearing down sensation, which accompanies the healthy protrusive effort. The membranes are not felt to become very dense during pain, and the woman experiences a general feeling of anxiety and restlessness during the intervals of the pains. "In this case, the loss of power is only relative, and though it may have all the appearance of absolute weakness, and is frequently mistaken for it, yet it is not truly so. The remedy in this case is, to remove the cause by evacuating the liquor amnii."

This apparent exhaustion of the uterus may also depend on an engrossed state of this organ. This state of the uterus may be known by the labour commencing and proceeding, for a time, regularly and properly; but the uterine powers becoming afterwards gradually weaker, until they cease to produce any propulsive effects on the child. "The os uteri is disposed to dilate; but the presenting part is not protruded during pain, and this pain is felt over the whole abdomen. The woman complains of a sense of suffocation or sinking; the pulse is hard, full, or depressed; and the pains are irregular, both in force and frequency. *This case is relieved by bloodletting alone*, so far as we have yet observed." The author details the following case in point:

"Mrs. ———, aged twenty-eight years, in labour with her first child; her pains commenced regularly, and pretty severely for some time, and then became desultory in frequency and force. The midwife, before we saw her, gave her some stimulating drinks, which increased her unpleasant feelings, such as a sense of suffocation, heat, and pain over the whole abdomen, sickness at stomach, &c. without augmenting the force and frequency of her pains. When we first saw her, she was labouring under all the distressing symptoms just mentioned, together with a depressed pulse, frequent sighing, great uneasiness and apprehension, the mouth of the uterus not much dilated, though quite unresisting when attempted to be stretch-

ed; its edges were thickened, but not tense; when in pain, very little impression was made upon the child, and the mouth of the uterus rather contracted than opened. She was bled, at first to the extent of about twenty ounces; but as she was only partially relieved, and as the pulse acquired vigour by the operation, the bleeding was continued until all the unpleasant symptoms were subdued, which happened upon the loss of about ten or twelve ounces more. The pains now increased very much, and in about twenty minutes more she was safely delivered."

There is another variety of exhaustion of the uterus, which it is of importance to understand. In this the labour commences briskly, the pains returning quickly, and with considerable force. They are attended with the usual favourable precursors of labour, such as a subsidence of the abdominal tumour, and the secretion of mucous.

"After all these favourable appearances have continued a longer or shorter time, the *pains cease altogether*, or nearly so, without any evident cause, and the whole labour seems to be at an entire stand. The pulse is very little disturbed, but a train of nervous symptoms supervene, such as palpitation of the heart, great oppression about the præcordia, with a sense of suffocation if the patient attempt to lie down, and a disposition to syncope if she rise up. The os uteri is well dilated, and the membranes remain entire. This condition of the uterus is peculiar, and requires the administration of such remedies as may have a specific action upon its fibres, such as the secale cornutum, and is one of the happiest cases for the illustration of its powers. Stimulating medicines or liquors are decidedly injurious."

The directions laid down by our author, for "*conducting a preternatural labour*,"—for performing the operation "*of turning in each particular case of head presentation*," and "*for the use of the forceps*," are precise and satisfactory, and may be studied, with instruction, by the old and the young, the experienced and the inexperienced. It is in this part of the work, particularly, we think, that Dr. Dewees has shown himself the experienced and enlightened accoucheur.

His chapter on "*Rigidity, &c. of the soft parts as a cause of preternatural labour*," is one, also, of much interest and value.

When there is much rigidity of the neck of the uterus, the

soft parts yield with great reluctance, and make the labour extremely painful and tedious. In cases of this kind, the most effectual and certain mode of disposing the parts to relax properly, is to bleed copiously, so as to produce an approach to fainting. The author introduces several cases illustrative of the beneficial consequences of this practice,—a practice which we have ourselves, in a few instances, employed with the happiest effect. To exemplify, more particularly, the effects of copious bleeding in difficulty of labour from this cause, we quote the two following cases:

1. "Miss V. aged fourteen years and a-half, was taken in labour, January 14th, 1790. She had been in pain thirty-six hours before we saw her; that is, she complained for that period, though the pains were not very severe; about twelve hours before we visited her, the waters were discharged; the mouth of the uterus was but very little opened, and the external parts not favourably disposed; the pains now were very severe, and the head was pressed pretty deep into the pelvis; she was extremely costive, and had passed no urine for many hours; an injection was ordered, which operated very freely; the catheter was introduced, and nearly a quart of water was drawn off;—she was much relieved by these discharges. An hour was given, in hope that a favourable change might take place in her labour. There was but very little heat in the vagina, for she had been rarely touched. She had, however, by the advice of the midwife, been placed over water, and fumigated with burning onion shells, but to no purpose.

"We now bled her fifteen ounces; this produced some little change in the mouth of the uterus, but not sufficient to permit the head to pass, as it contracted and stiffened with each pain. In an hour more she was again bled fifteen ounces; this produced sickness of the stomach, which was our signal for stopping. Upon examination now, the parts were found sufficiently dilated; there was a temporary suspension of pain; but they soon returned, and were of competent force, and much more tolerable; the labour was soon after terminated."

2. "February 17th, Mrs. ———, aged forty, in labour with her first child. She had been long in labour previously to our seeing her, and had suffered much pain,—her pains were in quick succession; the waters were still undischarged; the uterus opened to about the size of a quarter dollar; its edges very firm; no disposition in the external parts to relax. She was bled largely, (forty ounces) and was delivered in half an hour after."

The same mode of proceeding, according to the experience of Dr. Dewees, is equally effectual in cases where the soft

parts have become contracted, or formed adhesions, in consequence of some local injury sustained from the long continued pressure of the child's head in a previous labour, or from lacerated perinæum, or finally, from the "ill-judged use, and worse directed application, of instruments." Several highly interesting cases are given, in which obstacles of this kind were safely and speedily overcome by bleeding, *ad deliquium animi.*

"On the 26th of September, 1820," says Dr. D., "we were called to a woman in labour, in consultation with Dr. Ruan. She had been twelve or fourteen months in travail with her second child.* The pains were frequent and strong, the waters had been discharged some time; the head was favourably situated, and completely occupied the vagina; the perinæal tumour was large; the os externum did not exceed in size a finger ring, admitting the finger with some difficulty, in the absence of pain; during a pain it would be thrown up against the inferior edge of the symphysis pubis, so as not to admit the finger, or permit it to remain when it had been previously introduced. Externally, a large cicatrix was found to run to the very edge of the anus; internally, it could be traced farther. This seam prevented the unfolding of the external parts so effectually, that the repeated efforts of the uterus for several hours, were insufficient to make them yield, though the head had been closely applied to them for that period. The patient was a strong, healthy woman; considerable fever was excited; the pulse was strong, frequent, and hard. We proposed bleeding, ad deliquium, to which Dr. Ruan consented. A vein was opened immediately, and we took away about forty ounces of blood; but as her pains were very rapid, we were obliged to draw it from her while in a recumbent posture, and no disposition to syncope was manifested. This quantity, however, had some effect, as there was evidently a disposition in the parts to relax, and an abatement of the severity and frequency of the pains. A second bleeding was determined on, and to perform it while in an erect position. We effected this with some difficulty; but upon taking five and twenty or thirty ounces more, she fainted;—she was laid on the bed, and was in a few minutes delivered by the forceps, of a fine healthy boy. The patient recovered rapidly, without accident or draw-back. About three years we again delivered the same woman, by precisely the same means."

The chapter on *Uterine Hæmorrhage*, contains a well digested account of the most authentic information contained in

* With the first she had suffered an extensive laceration of the perinæum.

the books on this subject, interspersed with valuable original observations and facts, drawn from the author's own experience. We are particularly pleased with his remarks on the consequences which are apt to follow the retention of the placenta, after the expulsion of the fœtus in abortion, and with his mode of effecting its removal.

"In such cases," he says, "we have employed, with the most entire success, a small wire crotchet to bring it (the placenta) away. The manner of using it is as follows: the fore-finger of the left hand is placed within, or at the edge of the os tincæ; with the right we conduct the hooked extremity of the wire crotchet along the finger, until it is within the uterus, it is gently carried up to the fundus, and then slowly drawn downwards, which makes its curved point fix in the placenta; when thus engaged, it is gradually withdrawn, and the placenta with it. The discharge instantly ceases in every case where we have had occasion to use it. In every instance to which we here refer, we are persuaded that it preserved the woman."

On the subject of puerperal convulsions, Dr. Dewees' observations are very valuable. We regret that we have not room to enter upon a particular notice of his opinions and practice on this important subject. We trust, however, that there are few practitioners of midwifery in this country, who will not take an early opportunity of procuring and reading the work itself. E.

ART. VI.—*Reply to the Objections of the Medical Review, No. II. Art. IV. against Dr. Meigs' Lecture on the State of the Blood-vessels in Fever.*

Queris Ulysses ubi erraverit, potiusquam efficias ne nos semper erremus.
Seneca.

PEOPLE are generally both more fond and more capable of pointing out the errors of others, than of showing the true path of wisdom themselves; and this is one reason why the literary public has, in latter years, been favoured by the establishment of what are called Reviews; which being mostly conducted by eminent scholars, are received as nearly oracular in their commendations or denouncements of the several mat-

ters they take in hand; because these reviewers, in assuming to themselves the office of judges in *foro litterarum*, do, virtually at least, announce themselves as prepared by deep erudition to show in matters of science, not only Ulysses ubi erraverit, but what Seneca thought to be of far greater consequence, ne nos semper erremus.

A good review is then matter of thankfulness to the generality of readers, who, willing to take opinions at second hand, are thereby protected against all that loss of patience and waste of time, which would be incurred by the perusal of all the lucubrations that pour from the prolific sources of the press. A good review is characterised by a cautiousness which never commits itself; it never thinks it is shedding rivers of blood from the veins of misbelieving enchanters, while it really is assaulting the wine-skins from which it might have been nourished and enlivened; and why? Because it always sees with its eyes open, and never pretends to see in the dark; being aware of the great difficulty of seeing well, even in the best lights of learning. It generally admits all self-evident propositions;—it does not deny, at least, what is matter both of common credence and unequivocal demonstrability.—It knows that every reviewer, how didactical soever, is not Pythagoras, nor every reader his pupil, with the everlasting, ever-ready, magister dixit at his tongue's end; and is also aware, that among possible events, a reader might desire to peruse for his own satisfaction, an article which, passing unscathed under the sharp edge of his wit, had also come off unbruised by the bludgeon of his judgment and erudition. For in such a predicament, if a reader of Art. IV. should inquire where is Dr. M's. lecture to be found, he would be astonished to learn, that it had actually crept into a journal so respectable as that of the Medical and Physical Sciences, No. 14, edited by Professor Chapman. If Dr. Meigs', therefore, complaining of the unsatisfactory exposition of his theory contained in the review, should have preferred to leave the decision on its merits to readers in general, how polite

would it not have been to indicate to those at a distance, that the lecture had actually been printed, nay, was even extant.

We do not know that Dr. M. has any reason to complain of the temper of the review; for he is aware that those who communicate false doctrines, or make ridiculous applications of true facts in our important science, deserve ridicule, if not worse, reprehension. In the like good temper with that of the review, therefore, we shall proceed to enforce his doctrine by some additional arguments for its reasonableness, but more especially by showing, that the reviewer has not been able to refute those formerly advanced in Dr. Chapman's journal.

As the doctrine contained in the lecture, was of a character altogether novel, its readers were specially requested to examine it carefully, and, says Art. IV., "we feel ourselves invited to the task, &c." nevertheless, it seems he did not *accept* the invitation, for if any one should now ask him what is Dr. M's. theory, he would reply, it is this:—the *blood-vessels are weaker in the hot stage of fever than in health;* an idea of the theory which is not to be found in the lecture, so little attention has he paid to an essay he has undertaken to review, and to discredit.

He has not stated, or, perhaps, examined, Dr. M's. arguments at page 294-95, drawn from the antagonist character of all the operations carried on by vital contractions in the human body, without refuting which, he could not deny any of the inferences from them: he has totally neglected to show what are the true powers of arteries, or capillary vessels; and what is the nature of their action, or that of the heart.

From some of his assertions one would think he had neglected the very text-books of physiology, and he has certainly misunderstood parts of those he has quoted. This being the case, how can he say, we accept the invitation of the lecturer, since he has not qualified himself by careful study of the lecture, and other disquisitions, to appreciate the theory of Dr. M.

We shall quote here a part of the definition of the word "action," and refer for a fuller account of it to the lecture, or to the Dict. des Sciences, Med., where it is elaborately stated.

"When, therefore, I use the term action, in relating a supposed physiological or pathological condition of the heart, of an artery, or a vein, I do not use it to express the whole of that great vital function, the circulation of the blood, but only as hypothetic of the state of a single fibre, or all the fibres of the heart, of an artery, or a vein, or all the arteries, or all the veins. When I say action, I mean action, when I say function, I mean the function." p. 291.

Can a valid objection against this acceptation of the term be made? The reviewer acknowledges at page 217, that "if increased contraction alone constituted increased action of the heart, the proposition of the lecturer would necessarily be admitted." Now, if the whole of the doctrine which he was assailing, rested on this single point, it was his manifest duty, instead of rambling off after Bichat's untenable theory of the extension of organic muscles, to show wherefore it is, that contraction alone does not constitute action of the heart. The truth is, he has no reason for his objection, and I shall leave the matter here for the present, only with a reference to the article Action, in the Dict. des Sci. Med., where the physiological term is well defined.

To show how careless the reviewer has been in preparing his refutation, let us contrast the following sentences, in which, after charging Dr. M. with holding "incorrect views concerning the properties of arterial tubes," he announces, at p. 214, "Had he said, that in the cold stage of fever the arteries are more contracted than in the hot stage, he would have spoken correctly," whereby he tells us that Dr. M. has *not* said they are "more contracted." Again, "according to his notion, increased action of the heart means nothing more than *increased contraction;*" whereby he admits what the other sentence does not admit, viz: that the lecturer sup-

poses them more contracted in the cold stage, and therefore has "spoken correctly," as the review both does *admit* and does not *admit*. Now, as Dr. M's. paper was written to prove them more contracted, or more active in the cold stage, the foregoing will serve to show, that the reviewer did not read the paper seriatim, as a reviewer should do, or it will at least manifest the want of carefulness to which he felt invited: hence, Dr. M. has a plain right to appeal from the judgment of the reviewer, on this head alone: he has a much more just reason to appeal on the ground of want of clearness, in the exposition of his theory, and the arguments for it.

At page 293, the lecturer notices Bichat's arrangement of the systems of the body, and says,

"We find, as, for example, in the mucous tissue, a great uniformity of structure, properties, and application, from the knowledge of which (uniformity) we may draw the most important practical lessons; so, also, in the muscular, serous, &c. These are all governed by particular laws, which pervade every part of them, the law of one part being the law of every part of the same kind. Wherefore shall we not admit the same unity of properties in the circulating system? Have we not in our pathological reasonings, lost sight of the advantages to be derived from Bichat's arrangement, and separated too widely our ideas of the venous tissues, from those we have of the arterial and capillary, and all of them to an infinite distance from our ordinary notions of the heart? The heart and vessels constitute one single system, the heart is part of the circulating tissues,—it is an essential portion of that system of tissues to which it is attached and subservient, enjoying the same *kind* of vitality, dependent on the same sort of (ganglionic) nerves, therefore governed by the same laws, susceptible of the same exaltations and diminutions of actions with them."

On the next page is a reference to Bichat, for the circumstances which characterise different portions of this *system*, which the lecturer is, nevertheless, disposed to consider as an unit. We would now ask, with what face can the reviewer charge Dr. M. with sustaining this idea of the sanguiferous organs upon an "ipse dixit, he thinks so, and therefore it must be so," advancing no reason for this ridicule, ex-

cept that he does not *understand* what is meant by the unity spoken of, and which he yet proceeds to refute. Is there not then, evidently, a great lack of clearness and fairness in the exposition of Dr. M's. sentiments.

As Art. IV. did not condescend to point out the place where any reader might find Dr. M's. paper, in strict justice to that gentleman, he ought, in a clear and concise manner, to have stated Dr. M's. opinions, *with* some of his principal arguments.

May we not then here present an extract, in order to throw some light on those opinions.

"The *pathological* state of the veins is, I think, almost always referrible to a condition primary in the arteries.

"Both veins and arteries are constantly exercising a power, antagonist to that of the heart.

"In health they antagonise it perfectly,—in disease imperfectly,—either too much or too little.

"They of course antagonise each other. Both possess the power of diminishing their diameters, the length of which depends on the degree of antagonist force. Therefore, when they are small, it is because they antagonise with more force, more action; when they are large, because they do so with less force, less action.

"But they (the arteries) are small in a chill, and large in a fever.

"What do we see then in the large, round, full, pulse of fever, except the proofs of diminished resistance to antagonist power, and consequently the proofs of at least a relative debility." p. 295.

I will add of diminished action, and vice versa. The candid reader will obtain from the above extract a clearer idea of Dr. M's. sentiments than he can collect out of the whole Art. IV.

The arguments (if they deserve the name) at pp. 218-19, are only a series of sophisms, which can have not the least influence on one who takes the trouble to think, while he reads this physiological controversy. It is here, upon a strange misinterpretation of a plain and intelligible statement, that the reviewer builds up a most self-complacent refutation.

"How is this," says he, "the doctor has told us that in the hot stage the heart and arteries are in a state of (relative) debility, acting with diminished energy. If this be the case, how can the reduction of the mass of the blood, and an abstraction of stimulus restore the lost energy of the arteries, and terminate the hot stage? In the above paragraph, the author tells us, that in the sweating stage the momentum of the circulation is very great, —in this we agree with him perfectly, but how can he reconcile this fact with the principles of his doctrine. In the sweating stage, not only the arteries, but the veins, capillaries, and heart, are more dilated than in a state of health, and therefore, according to him, more debilitated."

We have quoted all this penetrative acumen, for the purpose of showing what the reviewer certainly regards as an inexpugnable position, but which, nevertheless, has clearly no manner of efficiency when compared with the actual sentiments of the paper; and if it has any appearance of strength or of appositeness, it lies in the erroneous supposition that Dr. M. has asserted the arteries, in the hot stage, to be in a state of actual debility, as compared with other portions of contractile tissues in the body,—and also in confounding the action of the vessel with its function, a distinction which the reviewer seems never to have made except by accident.

Let us put this matter in a clear light; Dr. M. has never stated other belief than the following: Marsh miasm, or the causes, whatever they be, of intermittent fevers, are of a stimulating nature. These stimuli excite the vascular system, the left, or arterial portion of which, is most easily excitable, or soonest capable of feeling the stimulation of the miasm; it *soonest* becomes more contracted, acts more powerfully, antagonises too much the right or venous portion of the system, and refusing to receive or contain the usual quantity of blood, accumulates it in the veins. Hence the small contracted pulse of chill. According to a known law of contractile and other tissues, excitement is soon followed by less excitement; therefore this excited, stimulated, active state of any artery, is followed by a state of less action or debility;—it is still, however, far greater than in health;—it is less, compared with its

antecedent amount, it is relatively weaker, because the veins which received the surplus in the cold stage, begins to restore the equilibrium of contractile phenomena in the sanguiferous system; they urge on the blood more rapidly to the heart, the cavities of which distended more completely, and yet contracting completely, throw out a larger column, making a larger pulse—throw it out frequently, making a frequent pulse—throw it out suddenly, making a quick pulse.

The action of the veins is here greater than in health, and we suppose was so even in the *congested* state, (with some exceptions) it was not *then*, however, equal to the action or power of the arteries. In the hot stage they are becoming balanced by the increasing exhaustion of the arteries, *i. e.* by what Dr. M. calls the *relative debility of those tubes.* In this balanced state, the whole sanguiferous system being equally pervious, and pressing more forcibly on the blood than it does in health, that fluid flies the round with great velocity.

This seems to us a very plain case. If we lay bare a long portion of artery in a living animal, it frequently contracts so much by the stimulus of air in dissection, as to obliterate its cavity. When Mr. Hunter divided the posterior tibialis in a dog, under such circumstances, the blood only oozed out from the cut end; will the reviewer charge this phenomenon on the debility of the artery? Will he call it diminished action,—a low pulse? Surely he ought, if consistent with his views.

I have seen an aged man die in the cold stage, I should say, of the cold stage of an intermittent; what is the real state of the case?

He shall have at six A. M., a good, round, tolerably full pulse,—not confined to bed: between six and nine o'clock, his pulse becomes more frequent and smaller,—at nine he is in the tremor of ague, his pulse is a thread,—it disappears entirely at the wrist, in the foot, at the bend of the arm, in the ham, and in a few hours he is dead,—why? Because, his ar-

teries and cutaneous capillaries have excluded a large proportion of the blood they ought to contain, and because the venous system of the head, thorax, and abdomen, are *consequently* actually gorged, and in a state of venous apoplexy so complete, that their great functions are interrupted.

He asks, "how can the momentum of blood be increased, when the force which moves it is diminished?" In the same spirit he might ask, how can convulsions take place in those who are dying of hæmorrhage?—We may suppose him to say, that convulsions can not be the penultimate symptoms of such persons, because just before death the weakness is great, but muscular strength is the expositor of force for the whole system; in convulsion, the muscles are strong, therefore there can be no convulsion;—or he might as reasonably deny, that a man walks better and faster in health than in convulsions, because his muscles are strongest in convulsions, or assert that a mill-wheel which is both drowned and has a great head of water on it, moves better than with its proper quantity above and below. If the heart is so much in action, so morbidly active that it will only give space for the reception of ℥ss of blood in its ventricle, it will surely not carry on the circulation as rapidly as if it will receive ℥ii at each diastole. The momentum, therefore, cannot be so great.

What is the meaning of momentum? The momentum is the quantity of matter multiplied into the velocity; the velocity has a relation to the amount of resistance,—the resistance is greatest in the cold stage. The moving force, that of the heart, being supposed the same. Dr. M. did not undertake to decide, nor can any one decide, the ratio of resistance, or velocity; suffice it, that the momentum is truly greater, because the resistance is less. Lastly, where has the paper stated the moving force to be diminished? No-where.

Having now made manifest such of Dr. M's. sentiments as might in vain be sought for in Art. IV., we think he will have an admitted right to appeal from the judgment of the

reviewer, on the ground of want of clearness and fairness in the exposition of his sentiments.

This being done, it will be lawful now to show some of the numerous mistakes which abound in Art. IV.—lawful, because on these much of his criticism depends,—lawful, furthermore, because in proving his errors we shall prove the soundness of the opinion we are upholding.

"The action of a circular fibre, as the author *correctly* observes, tends to lessen its diameter, and, on the contrary, its relaxation must increase its diameter." p. 213. In this truly Delphic sentence are contained the two following propositions:

1*st*, The action of a fibre tends to *shorten* that fibre, which is very true, and is all that can be said or supposed on the subject.—2*d*, The *action* of a fibre must *increase* its length. If any thing intrinsic to the fibre acts, it only shortens it,—very well: if any thing extrinsic acts, it is not the fibre that acts, but something else: "but," says he, "its relaxation must increase its length;"—why? Because the relaxation which he speaks of, is an active, vital, and intrinsic movement, according to him;—he even pretends that the heart can spring open with a wonderful force,—hence he thinks the diastole an *action* of a circular fibre, which "must increase its diameter," —but has he not just praised Dr. M. for correctly observing that the systole is the only state of action?

We hope Dr. M. will never say the relaxation of a ring muscle, "must increase its diameter;" he never can say it very correctly, it would be incorrectly, for a muscle is not elongated as soon as relaxed;—it is relaxed whenever volition or stimulus cease to keep it in action. Nothing but an elongator can ever restore it to its greatest length. Elongators are required both for long and hollow muscles. Action depends on something intrinsic to a contractile fibre, relaxation is the cessation of action,—a negative elongation is effected in every case by something extrinsic, and cannot be its action, or any part of it. How completely then has he misapprehended the use of this physiological term.

Dr. M. supposes (p. 301,) that no straight or circular fibre of the body can physiologically lengthen itself, but only shorten itself. That the heart is a hollow muscle, whose only *action* is the contraction of its fibres, or diminution of its cavities;—that when most excited, most in action, it most resists the impulse of the veins, and receives, consequently, less blood,—in a smaller ventricle, and why smaller? Because, the hollow muscle excitable sooner, and sooner excited, discharges itself sooner,—is more active in throwing off its blood, —more in action. The arteries partake of this pathological condition. Can any one object to this? Or is action, in our sense, any thing more than that movement produced by the agency of excitants on excitable matter;—the action being greater in the ratio of the excitant, or the excitability, or both.

Says Art. IV., "were it true, as he supposes, that increased contraction alone constituted increased action of the heart, then, indeed, we should be obliged to admit that a small pulse is indicative of increased action, at least of the heart, if not of the arteries." And has not the reviewer admitted, as before quoted, that "the action of a circular fibre, as the author *correctly* observes, tends to lessen its diameter?" Now we are inclined to think, that if the action lessens diameter, increased action lessens it still more, or contracts it still more. We recommend some of the reviewer's "daylight," for the express benefit of this part of Art. IV.

Mr. Hunter, whose judgment in matters of physiology was nearly unquestionable, because of the great carefulness and coolness with which he judged, as well as because of his great genius and penetration, has made a comparison of the phenomena of the circulation, produced by *exercise* and by *disease*;—he says:

"And if a greater quantity (of blood) be thrown out, (from the heart) and with a greater velocity, then the arteries must *relax* in proportion,—since the different parts must correspond with each other. We must therefore

suppose, that in *health*, whenever there is any exertion greater than common, which always increases the pulse, the heart dilates more, and contracts more, and does both with greater velocity,—this arises from a necessity which I conceive begins first in the veins." p. 125.

Exercise of the muscles tends to urge forward the blood in the veins, causes it to arrive in large quantities at the heart, which being in a state of natural, not morbid excitability, allows its ventricles to be disturbed fully before it acts,—it takes in a large quantity by a perfect diastole, after which it acts, and lets out a large column into the vessels, which being also in a healthy condition, are ready for the increase of circulatory function. Hence the large artery from exercise; and in this case Mr. Hunter justly observes, that the necessity is primary in the veins. But, proceeds he,

"Does the same thing take place in disease? Does the heart dilate more, contract more, and also contract with more velocity? I believe this case does not in the least correspond with our former position. *The pulse, in such circumstances, although frequent, is small and hard, showing the arteries to be much contracted by their muscular power, and therefore unfit to receive a large quantity of blood from the heart in a given time.*" p. 125.

It would be difficult to give a clearer expression of Dr. M's. theory of the state of the blood-vessels in fever, than is contained in the above extracts from the treatise on the blood: we only object to the supposition, that the velocity is less, (*i. e.*) the contraction less sudden;—we apprehend the action is much more sudden.

The description of the phenomena of the heart and vessels, as arising from exercise, hardly differs from Dr. M's. account of them in the hot stage, between the phenomena of which, and those arising from exercise, there is often the strongest analogy,—nay, sometimes even identity. The lecture supposes the hot stage to depend on the increase of circulation, produced as in exercise, by the increased relative power or activity of the veins. The cold stage depends on the increased power or activity of the arteries, which, to use the

language of Mr. Hunter, are unfit to receive a (sufficiently) "large quantity of blood in a given time."

With what face, then, can the reviewer say, he has seldom witnessed a doctrine more void of solid foundation? Is the sanction of John Hunter's profound judgment of no value, or does it afford no semblance of strength to any argument? Will he show how it is not true, that contraction is the only *action* of the heart? Or does he now feel "obliged to admit, that a small pulse is indicative of increased action, at least of the heart, if not of the arteries?"

To show how our reviewer flounders in the sea of physiological doubts, on which he has embarked, take the following:

"The heart, however, is not passive in its dilatations,—the *action* of dilatation is not merely a remission from *action*, but as much the effect of *active* powers of the heart, as its contraction." p. 218.

"Yet action," as the author correctly observes, "tends to lessen its diameter."

The notion of an active diastole, which was exploded so long ago, he certainly would not have brought to prop up his sentence delivered against Dr. Meigs in the very outset of Art. IV., unless that heedlessness which distinguishes his review, had also attended his study of Bichat. Let him think as well as read;—if he had given one sober thought to Bichat's article on the phenomena of relaxation in organic muscles, he would have perceived that that great man was led by his passion for analysis and refining, in direct opposition to reason and fact; that each, and every one, of the six reasons advanced by him in support of his idle doctrine, is alike destitute of foundation; and finally, that the whole error has arisen from a careless performance of a very common experiment. For example, take two living hearts, thrust your finger into the ventricle of one, while in the other hand you grasp the other heart. "You will feel," says Bichat, "that the latter

makes as great an effort in dilating, as the other in contracting."

He goes so far as to say, that the effort of dilatation is "even sometimes greater than that of contraction."

It is most surprising that so acute a physiologist could have overlooked the real state of the case, as he has done, in mistaking for the diastole, what was in reality the systole of the heart. When he took it into his hand, it was in a state of relaxation, it was flaccid and compressible; when it contracted, it became suddenly shortened, and thickened in his hand, forced his grasp in some measure open, and gave to his careless observation the *appearance* of diastole, when it was the systole he was remarking.

Sprengel, in his Histoire de la Medicine, charges Wm. Harvey with holding this same doctrine of active dilatation: I have sought in vain for such an opinion in the writings of that sensible physiologist, certainly it is not in either of the Exercitationes Anatomicæ.

On the contrary, in that ad Riolanum, he says, "Pateat quod in ventriculos sanguis ingrediatur, non adtractione aut *extentione* cordis, sed pulsu auricularum *immissus.*" In another place he expressly corrects Cartesius, who had fallen into the same error with Bichat and our reviewer, of *mistaking* the systole for the diastolic state, and assigns the true cause of the latter, namely, the antagonist power of the auricle. 280. Harvey says again, "neque in diastole vigoratur cor, sed in systole;" again, "neque verum est quod vulgo creditur, cor proprio suo motu aut extensione sanguinem in ventriculo adtrahere, sed dum *movetur* et tenditur *expellere*, dum laxatur et concidit, recipere sanguinem." ap. Manget Biblioth.—I. 846.

Old Francis Glisson, perhaps the most profound and erudite investigator of early physiology, says, "Actio ergo Fibræ, sive motus activus, duplex est, contractio et relaxatio; sed hæc (relaxatio) minus proprie actio audit, *consistit enim in remissione actionis, et reditu ad nativam figuram.* Quare,

quando de *actione* fibræ indiscriminatim loquimur, semper intendimus *vigorationem* ejus et *tensionem* sive CONTRACTIONEM, non relaxationem," De Vent. et Intest. cap. v. One would think that this standard author gives abundant support to D. M's. acceptation of the term, and his application of it.

Haller says, that in relaxation, the heart is "leve planum et molle," Phys. I. 387. Also, "Post brevem systolen diuturnam diastolen succedere in qua cor *molle laxum et quietum* manet." There are many such places—he expressly refutes Pechlin.

Mr. John Hunter, looking at a heart during artificial respiration, observed, "that the ventricles were not turgid at the time of their diastole, for I could *feel* them *soft*, and could *easily compress* them."

Is it not then useless to pursue any further the proofs of our reviewer's error. They are every where abundant, and seem to be fixed almost intuitively in the mind of every student. How could he then so didactically announce "unquestionably, *therefore*, an increased action of the heart consists both in a more vigorous contraction, and (vigorous?) dilatation (qu! relaxation?) of this organ." 28.

We beg leave here to remind him of his promise to admit D. M's. theory "if it were true that increased contraction alone constituted increased action of the heart."

Let us now proceed with our plan of reply, by showing his errors, and therein upholding our own doctrine on the properties of arteries, concerning which, he gravely charges Dr. M. with holding "erroneous views." Here is an apposite dictum. "In short, their principle of contraction is totally distinct from that of the heart, being the consequence, not of an excitation of muscular contractility, but of an *elastic* power inherent in the arterial structure, by virtue of which these vessels contract and dilate, according as they are distended by mechanical force." It is hardly possible to suppose a reviewer unacquainted with Mr. Hunter's standard work—what then does he understand by the three states in which arteries

are found—vid., the natural pervious or middle state, in which it is placed by the elastic coat; the stretched state, or distended state; and the contracted state which it acquires by the action of its contractile fibres; fibres, which in vessels of moderate size, are able completely to obliterate the tube, as in Mr. Hunter's experiment on the posterior tibial artery of a dog; on the umbilical arteries, &c. This contractile coat is the only one of which physiological action can be predicated, and in which only it is observable. Are not Mr. Hunter's experiments conclusive, are not his remarks, from p. 90 to 108, absolute demonstration of the active, vital contractility, nay, I will say, even muscularity of the arteries? Parey's work is full of demonstrations on this head, he has often seen the artery contract on exposure; in one case it contracted as if a string had been drawn round it.—Exp. 18. We have no room to quote here more authorities—it is also not necessary, as the above may sufficiently reprove his hasty charge against Dr. M., of "holding erroneous views," &c.

We have striven, but cannot resist the temptation to expose him in the following:

"The principle of contraction is an elastic," &c. &c. "This elasticity is what Bichat calls the contractility of texture." p. 215. What Bichat calls the contractility of texture!! "L'elasticitè des artères," says that physiologist "est aussi remarquable apres la mort que pendant la vie; il est *essentiel de la bien distinguer de la contractilite de tissu.*" Among many reasons for this distinction, he says "3d. tout mouvement elastique est brusque, soudain, aussi prompt à cesser qu' etre produit, au contraire, tout mouvement de contractilitè de tissu est insensible, lent, dure, souvent plusieurs jours."—Anat. Gen. I. 390—91. He also calls it contractilite par defaut d'extension. It is Francis Glisson's perceptio naturalis; if it is not Haller's vis insita, I know not where to place it in the system of that author, for it is surely neither the vis in genere, mortua, or nervosa, of that philosopher; it is muscu-

lar action, produced by the stimulus of necessity of John Hunter.

We confidently hope the reviewer will now give up his reliance on that elasticity which "Bichat calls contractility of texture," and of course give up the idea of a refutation to be founded on such "erroneous views concerning the properties of arterial tubes." We might also here address him with the old apophthegm, de te mutato, &c.

Has the author of Art. IV. ever seen the transverse section of an artery? Has it not always presented the appearance of a ring? This ring is no longer or smaller than the unobstructed elasticity of its "*membrane propre*," which we call its elastic coat, would make it: if you press it flat, its elasticity restores it to its circular state, just as a ring from a bottle of couatchoue would, by a physical force, totally unconnected with vitality, or its phenomenon action. If the ring was even larger during life, it was made larger by the injecting, distending force of the heart's systole; if smaller, it was because of the *action* of its contractile coat, not of physical elasticity, which keeps it open,—or in the middle state. It is by the combined operation of its elastic coat and contractile fibres, that it resists or antagonises the heart in ordinary health: but in disease, where the heart is stronger, or the vessel weaker, we have a large pulse, the antagonisation being dependent more entirely on the elastic coat—but if the vessel is stronger by some morbific stimulus, its contractile fibres operate, both against the heart's force, and the elastic coat, which latter it renders within or below the middle state. In this manner it may antagonise the heart; and by thus resisting the intromission of a large column of blood, give us a small pulse from increased action of the vessels.

We will now proceed to the exposure of more of our reviewer's errors. They abound in his disquisition on the capillary vessels, of which at p. 216, he says, "their dilatation and contraction depends on a principle altogether different from that which determines the same changes in the arterial

tubes."—Again, "They dilate or contract altogether independent of the heart or arteries. In inflammation, for instance, they dilate, either because they are debilitated, or because their organic sensibility, according to the ingenious theory of Bichat, is changed." &c. &c.

Now, if a circular contractile fibre can only shorten and not lengthen the diameter of a vessel, what becomes of his surpassing assurance that "their dilatation is altogether independent of the heart;" the answer is plain, he is mistaken q. e. d. And further, to "the ingenious theory of Bichat," the ingenuity of which the reviewer seems not to have apprehended, is it not evident, nay, strikingly so, that any change of organic sensibility of capillaries producing or permitting dilatation of them, can be only a change—from what? from the strength which can resist, to the weakness which can not resist the *intromission* of red globules, which are therefore forced into them by the vis a tergo, so unwisely rejected by Bichat and his hopeful pupil, our reviewer, after him, vid. Anat. Gen. I. 26. et. seq. Let us not be gainsaid, as here mistaking sensibility for contractility, we are not so misapprehensive; an increased sensibility must tend to *increase* the aptitude of the capillaries to exclude, not to *diminish* that aptitude.

Our readers will perceive then, that the "ingenious theory" does not save him. By a slight analysis, it is reduced to the true doctrine of dilatation from debility, actual or relative, the heart being the antagonist or elongator; the simple reading of the passage is its reductio ad absurdum: and we cannot but smile at the solemn annunciation, that the capillaries, which "*act* by an inherent principle of *contractility*," 216, are, "in every inflammation, *dilated*, and, if you please, *debilitated*," 217; while "in fever, on the *contrary*, the arteries are enlarged, not by their own *action*, but by the larger quantity of blood *forced* into them, &c. &c." 218. Now, if dilatation follows weakness, why does it follow it? Because, as before said, there is forcible injection by the action of the heart; how

marvellously quidnunc does the "on the contrary" therefore appear.

As to the reviewer's remarks on the practice resulting from the doctrine of the lecture, we shall observe that they are the erring products of his misunderstanding of Dr. M's. paper, attributable, we presume, entirely to careless reading thereof, so manifest throughout the IVth Article. Had he given a little attention to a paper he has been willing so utterly to condemn as ridiculous, he would have seen the theory of the author is simply this: in fever, the *system of sanguiferous tissues* is directly stimulated, instead of being debilitated, as is generally supposed, by the miasmata of marshes. This system being an unit of tissues like as the mucous system, yet is characterised by some circumstances of difference in different parts, all however possessing contractility, and able, during health, to antagonize each other so justly, that no morbid accumulation of blood shall take place any where; but able, under other conditions of stimulation or disease, to act or contract in an undue and morbid degree, and thereby destroy the regular proportion of the healthy and balanced circulation.

The vessels to the left of the heart, (divided according to Bichat's view,) are very excitable;—easier thrown into excessive or morbid action, wherein they refuse their regular proportion of function, when they act most violently, causing the greatest interruption of the function. In such cases we have little blood in them, and much in the veins, a venous congestion or engorgement, a chill. The veins, by a gradual and steady resistance, tend to overcome this disproportion; they are able to effect it as soon as the arteries, in consequence of the necessary exhaustion of their superior contractile force, come to be on a level in this respect with other portions of the sanguiferous system.—This contraction (action) of the veins, urges the blood more rapidly towards the heart, which in a given time takes in more, and throws out more than in the common healthy state, and far more than in the antecedent chill stage; so as that the congestion gradually dis-

appears; the circulation becomes more free and unobstructed, a great development of animal heat takes place; it is the hot stage. Dr. M., with great propriety, maintains, that a full pulse here argues debility and decreased action of arteries, "relatively to an anterior state of those tubes," as he says; since their state of dilatation is the effect of a mere mechanical distention, pushed so far exactly, as the vital contractile fibres of the artery and its elastic coat will permit, the dilatation being the inverse measure of *action*, the direct measure of weakness.

If we might suppose a sentence from Mr. Hunter, it would be, the phenomena of the cold stage are primary in the arteries—those of the hot stage are "primary in the veins."

Physicians rarely sit more than fifteen or twenty minutes by the bedside of the patient. They have not, therefore, examined closely the appearances of chill, which last sometimes two hours. Those who will examine the state of the pulse in ague every five or ten minutes throughout, will be apt to come over to our opinion.

On the 23d Nov. at Magdal. Asylum, I saw C. Smith, æt. about twenty-eight—has had several paroxysms of intermittent—the fit had lasted fifteen minutes when I called. My watch had a quarter second hand: at four minutes before 12 pulse 108.

past	12	128. Small, sharp, hard or corded, she is very chilly; shakes.
	12	136. Smaller, hard, she shakes, teeth chatter; ears, nose, and fingers cold.
15	12	128. Pulse not easily compressible, very quick; shaking and rattling of teeth together.
18	12	136.
20	12	140. Shakes violently.
22	12	140. Head begins to ache, says her back has ached sometime; has now some sensations of returning warmth, yet shakes violently with ague; teeth chatter.

25 past 12 pulse 140. Pulse very small, yet hard, not *soft*, nor easily compressed; has nausea.

30 12 124. Still shaking; pulse seems to be larger; heat of the body begins to increase.

35 12 128. Not so much shaking.

40 12 148. Shakes more than ever; her chill, which appeared to be diminished, seems to have returned with more intenseness.

45 12 120. The heat of the body more considerable; has head-ache and nausea. As I wrote the last remark she began to vomit.

I now left her with particular request that the matron would inform me of the period at which the chill might be considered as gone.—The next day I was informed that it ceased at one o'clock, (i. e.) in about fifteen minutes.

To return. We see no inconsistency in the theory, as applied to hectic and typhus.

Hectic is emphatically a disease of the arteries, and to consist with Dr. M's. idea, should be marked by a small and frequent pulse: Is it not so marked?

What is the relative condition of the right and left portions of the sanguiferous system in typhus? Is not there a preponderance of that fluid in the veins? But Dr. M. has not been obliged to admit that the arteries act violently in typhus, much less in syncope. In the latter case he believes they *sometimes do*, and thus produce that phenomenon—not always, nor often. He is aware that the heart may be really debilitated—that it may entirely cease from acting, as in some cases of syncope, in which the heart, not properly susceptible to the stimulus of the blood, may receive two or three ounces, and not throw out two drachms; the artery, however, would close on the two drachms as well as on the two ounces—because its contractile fibres constantly tend to the utmost contracted state of Mr. Hunter. Dr. M., therefore, may explain syncope on the ground of weakness of the heart, and consistently

show the small pulse of that disease to depend, not on an *actual* increase of arterial strength, but on a diminution of the elongating or impulsive effort of the heart, the veins remaining, as to action, unaffected.—Here the reviewer has a fund of arguments against Dr. M., and it was only from arguments collected here, that he expected to see his doctrine attacked, and perhaps overthrown.—It is not inapposite here to repeat, that the lecture was "on the state of the blood-vessels in fever," not on syncope.

The theory teaches us to avoid bleeding in the cold stage. "In our humble opinion" says the IVth article, "the true tendency of this doctrine is to lead to, rather than deter from the adoption of such a practice."

The opinion may be truly "humble" as he says, and we will not gainsay him; especially as we have shown so many of his errors in physiology and pathology. The therapeutics is, that as in the cold stage the arteries are in a state of violent action, the loss of blood will not have the least tendency to alter the *relative* conditions of the right and left portions, or the systems of red and black blood, as they are denominated by the "ingenious Bichat." If one were mad enough to bleed in this stage, would he lessen the quantity of blood in the veins, and not also in the arteries?

Moreover, what he calls "congestive forms of fever" are not cold stages of intermittents, though analogous to them. In their commencement, when the disproportion of action is not great, they bear bleeding—the practice is rash in more advanced states. This doctrine teaches us not to give brandy and red pepper in pleurisy, &c. "Why not," says Art. IV. "if he is true to his theory, are not these articles stimulants? —yea verily; but least they should act as such on the already stimulated vessels, and not by some "ingenious theory," select the veins for their special impression, we will have none of them, until the softness of the pulse and its volume give reason to believe, that debility in the whole sanguiferous system, and other parts, requires their use.

Furthermore, we deny that the lecture any where hath it that a "round, full, and *hard* pulse is indicative of arterial debility" 221, (i. e.) actual debility. Dr. M. observes, that the pulse, noticed just before the chill, is sometimes characterised by hardness, as is natural when the vessel begins to antagonise too powerfully the dilating effort of the heart, and resisting effort of the capillaries and veins.

Finally, we appeal from the judgment of this reviewer, and disclaim, for Dr. Meigs, such "daylight" as he has shed down in Art. IV. We close, by advising him to study the Physiologists, and commend to his special attention, that passage of the Anatomie Generale, in which the "ingenious" Bichat says, this *elasticity* is what I call "contractility of texture."

M.

Remarks on the preceding Reply. By the Reviewer.

That Dr. M. should not relish our strictures on his *theory*, is quite natural. In this we see but the influence of that generous impulse, which urges all beings to cherish and defend their offspring, however feeble or deformed they may be. We have no reason to be displeased with this trait of good feeling in any one,—much less in one who is so delighted with his little darling, as to fancy it of sufficient consequence to be committed to the fostering care of one of the first physiologists of the age.* It is doubtful, however, whether the Doctor has acted wisely in bringing it again into the field. If he had reflected on its weak and sinking condition, he would not have exposed it to unnecessary danger, but kindly administered enthanasial remedies, and suffered it to die in peace.

* The Doctor has dedicated his "Lecture" to Wilson Philip. Should this gentleman, through kindness, take it under his protection, he will find himself much in the situation of the nurse, in these lines of the poet:

"The tender infant, meek and mild,
Fell down upon a stone;
The nurse took up the squeaking child,
But still the child squeaked on."

The doctor, however, is not merely on the defensive in the above reply. He has advanced upon us right bravely, and seems to be exceedingly pleased with his success. It may nevertheless be well to show that his blows, though aimed with great deliberation, do not take the effect which one might suppose from the earnestness and apparent air of triumph with which he deals them out.

To show that we did not read his paper carefully and "seriatim, as a reviewer should do," Dr. M. endeavours to fix upon us a contradiction, which, as it is not to be found in our review, must be referred to his own imagination. He thinks that we are guilty of a great contradiction in saying at one place, that he ought to have said the *arteries* "are more contracted;" and at another, that according to him, "increased action of the *heart* means nothing more than increased contraction." We will quote, from our review, the two paragraphs to which his shrewd criticism refers: "His first and fundamental position, that in the cold stage of fever the *arteries* contract with *more force* than in the hot stage, is erroneous. Had he said that in the cold stage of fever the *arteries* are *more contracted*, than in the hot stage, he would have spoken correctly." Again, when speaking of the action of the *heart*, we say: "According to his notions, increased action of the *heart* means nothing else than *increased contraction.*" In the former of these two passages, therefore, we state what, in our opinion, he *ought* to have *said*, in relation to the *arteries;* and in the latter, what his *notions are* concerning the action of the *heart*. The doctor's critical dexterity must be much admired if he can show any contradiction in this.

The doctor charges us of not having been "clear and fair" in our account of his theory. If we have been deficient in this respect, we are sorry for it; for we certainly intended to be both clear and fair in our exposition of his doctrine. We are not conscious, however, of having failed in our duty on this head, and we therefore leave this point for the decision

of the reader, after having read our review, and the doctor's own "exposition," in the above reply. His object in this exposition, (page 411,) is to show, that the circulating system is an *unit* of tissues, enjoying *in all its parts* the *same kind* of vitality. He asserts that we do "not *understand* what is meant by the *unity* here spoken of." If the doctor has wrapt up some very profound and occult meaning in his phraseology, we own that we have not yet succeeded in getting at it; but if he means what he says, which may admit of some doubt, we do think that we know what his "unity of the circulating system" means. The doctor's reasoning in support of his position, that the circulating system is an unit of tissues, and that it enjoys the same "kind of vitality" in all its parts, is at once vague and illogical. "We find," he says, "as, for example, in the mucous tissue, a *great uniformity of structure*, properties, and application, so also in the muscular, serous, &c. These are all governed by particular laws, which pervade every part of them, the law of one part being the law of every part of the same kind. Wherefore shall we not admit the same unity of properties in the circulating system?" This argument rests upon the assumption that the circulating system possesses, like the mucous tissue, "*a great uniformity of structure*," which is by no means the case. The mucous tissue may with truth be said to be an "unit" in relation to its structure; but this cannot be said with equal correctness of the sanguiferous system. The heart differs very widely from the arteries in its tissues, and these again from the veins. The heart is almost entirely composed of muscular substance, whilst it is still a matter of dispute, whether the arteries contain any portion of this structure whatever, except, perhaps, in the aorta near the heart, and in the capillaries. "The proper coat of the arteries," says Bichat, "is exclusively peculiar to them, possessing no relation to the other systems, but forming a distinct and separate texture in the animal economy." (Anal. Gen. V. I. p. 307.) To make

out his argument, the doctor observes: "the heart and vessels constitute one single system, the heart is part of the circulating tissues,—it is an essential portion of that system of tissues to which it is attached and subservient, enjoying the same *kind* of vitality, dependent on the same sort of (ganglionic) nerves, therefore governed by the same laws, susceptible of the same exaltations and diminutions of action with them." Dr. M., no doubt, feels perfectly satisfied with this reasoning, but he must excuse us for considering it exceedingly futile. That the heart and vessels constitute "a single system," when viewed in a general way, we can readily admit; but that they form a "single system," when considered in relation to their structure and particular functions, we are by no means disposed to allow. The heart, arteries, capillaries, and veins, all co-operate in circulating the blood; but these distinct sections of the general sanguiferous system, perform very different parts in this important function. While the heart acts upon the blood, as a propelling power, the larger vessels serve, perhaps solely, as conduits for its conveyance. The position that these parts "all enjoy the same *kind* of vitality," is a mere assertion, and has, in fact, no intelligible meaning. The doctor seems, indeed, to be fully aware of the above objections to his doctrine. In his lecture he says: "I would not have you suppose me ignorant of many circumstances of difference, in different portions of the vascular system. Bichat has pointed them out,—but *they cannot prevent me* from considering the whole as an unit of tissues." We are perfectly satisfied that nothing could prevent him from adhering to his opinion. His theory absolutely requires him to be pertinacious on this point; hæc mea sunt, teneo.

The doctor considers our remarks on his explanation of the sweating stage of fever, as a good specimen of "*penetrative* acumen." He must be wrong; for we had not the remotest idea of exercising any "*penetrative* acumen," upon so *superficial* a thing. It may be well, however, to show

that with whatever "penetrative acumen" we may have examined his doctrine, our remarks on its errors have not suffered much from his observations. According to the doctor's theory, miasmata stimulates the heart and arteries,—these contract, and push the blood into the veins, and this forms the cold stage. "If the arteries, in consequence of their *increased* action in the *cold* stage, should have their energy reduced by exhaustion to a level with the veins, or should the veins, by the stimulus of distention, recover their superiority, we shall next perceive the evidences of their reaction," that is, the supervention of the *hot stage.* "But what is the third stage? I answer," says Dr. M., "that the natural termination of the foregoing condition, is to be looked for in some evacuation, which, by diminishing the quantum of fluids, and removing irritating recrementitial particles retained in the former, may reduce the mass, momentum, and stimulus." Upon this passage we remarked in our review: "the doctor has told us, that during the hot stage, the heart and arteries are in a state of debility, acting with diminished energy. If this be the case, how can a reduction of the mass of the blood, and an abstraction of stimulus, restore the lost energy of the arteries, and terminate the hot stage." In order to get over the evident contradiction involved in the passage to which these remarks apply, he tells us in the present "Reply," that although the action of the *heart and arteries* is much *less* in the hot than in the *cold* stage, yet "it is still *far greater* than that which exists during perfect life." And hence he would infer, in contravention of our objection, that the reduction of the mass of the blood, and the abstraction of stimulus, is necessary to bring their action down to a healthy standard. We ourselves believe, that the action of the heart and arteries is *far* greater in the hot stage of a fever, than in health, and that the reduction of the mass of the blood, and removal of stimulus, is necessary to bring them down; but we perceive in this admission, a palpable inconsistency with the spirit and true import of our author's reasoning and principles,

and it was with a view to expose this inconsistency, that we introduced the above passage into our review. In order, however, to show that Dr. M. has no right, according to his own principles, to assert that the *arteries* act with *far* greater force in the hot stage of fever, than in health, we need only refer to the reasonings he uses in his lecture.—He there tells us, that when an artery *acts*, it "diminishes its tube." That in proportion as any muscle *is stronger*, in a physiological sense, so will its *action be stronger.*" If, then, the *action* of an artery *diminishes its tube*, its *deficiency* of action must be attended by its *dilatation*, and this *dilatation*, cæteris paribus, must be in proportion to the degree of *deficiency* of the action. Now, if the action of the heart and arteries be *far greater* in the hot stage of fever, than in health, their tubes, according to the reasoning of Dr. M., should be *smaller* or more contracted than in health. But is this the fact? Who does not know that in almost every instance the pulse in the hot stage of an intermittent is more voluminous than in health? We may observe here, that this is a new *dictum*. In his lecture he does not say a word about the action of the arteries "being *far greater* in the hot stage of fever, than in health." He has found it convenient to adopt this idea, in the above reply, in order to disengage himself from the perplexity into which our objection threw him. He speaks, indeed, of the *relative* debility of the heart and arteries in fever; but in other places he maintains the broad and unqualified position, that the heart and arteries act with more force in the cold stage than in the hot; and that they are dilated and debilitated in fever.

The doctor's theory rests solely upon the two following positions: 1*st*, A small pulse depends on *increased action* of the heart and arteries. 2*d*, Marsh miasmata, in their primary operation, excite the vascular system to increased action. With regard to the first of these positions, its fallacy is easily shown. In approaching syncopy, the pulse is small; but will any one say that the heart acts more vigorously in this state? Bleeding or cold, or a nauseating dose of antimony, will produce the

same effect; yet we presume that even Dr. M. will not say that these causes increase the action of the heart and arteries. A *small* pulse is, therefore, no sure evidence of an increased action of the heart and arteries; and hence the doctor's position is not sufficiently established; for it rests entirely upon the testimony of this symptom, which, as it is fallacious, will afford it no certain support. The fact is simply this: If the heart be debilitated, its action becomes diminished; it sends out less blood into the arteries, and these, being endowed with an *elastic* power, by which they accommodate themselves to the quantity of the contained blood, assume a size corresponding to the quantity of blood which is driven into them by the heart.

The assertion that marsh miasmata stimulate the heart to increased action, as a primary effect, is not supported by a single fact. It is a mere *assumption*, predicated upon the previous assumption of the increased action of the heart, in the cold stage of fever; for having once determined that such is the case during the chill of a fever, he would of course ascribe a stimulating, and not a debilitating power to miasmata.

Having established, in his estimation, the correctness of the position, that the heart and arteries are in a state of *increased action* in the *cold* stage of fever, he could find no difficulty in raising the superstructure of his theory; for he had but to ascribe to them a subsequent debility, in consequence of their previous great action, and assume a contemporaneous increase in the action of the veins, for the purpose of pushing the blood into the debilitated arteries, and he had the hot stage. Now, although the state of dilatation of both the veins and the arteries in this stage, is generally *greater* than in *health;* and although, according to the author, dilatation is synonymous with relaxation, or *diminished* action, yet, the author finds it necessary, in order to put a consistent *sweating stage* to his fever, to assert that the action of the arteries and veins is, notwithstanding, greater than in health! For how

would it otherwise do to say, that the hot stage is put an end to by "the removal of irritating recrementitious matter and *stimulus?* The fact is, such a removal of recrementitious and stimulating matter does take place; and although the veins and arteries are more dilated than in health, and therefore, by his theory, more *debilitated*, yet our author found it necessary, upon second reflection, to ascribe to them great strength of action, in order to make his explanation of the third stage consistent, and obviate the objection we have already made on this point.

Dr. M. thinks we asked a very silly question in relation to his opinion concerning the momentum of the blood in the sweating stage of fever; and very kindly offers us some information as to the import of the word momentum. To show, however, that the question we asked, is a correct one, and such a one as Dr. M. cannot answer in consistency with his theory, we extract from our review the passage in question. Dr. Meigs', in his lecture, says, "the momentum of the circulation is now (in the third or sweating stage) very great, for the arterial, capillary, and venous systems, and the heart, are now equipoise; the whole system is pervious, being equally free for the passage of the blood, with a heart beating oftener and throwing out more blood than in a state of health." Upon this paragraph we observed: "how can the fact, that the momentum of the circulation is very great, in the sweating stage, be reconciled with the principles of our author's doctrine? In this stage, not only the arteries, but the veins, capillaries, and heart, are more dilated than in a *state of health*, and therefore, according to the reasoning of our author, more *debilitated.* How, we ask, can the momentum of the blood be increased, when the *force* which moves it is diminished?" Now, as the heart, arteries, and veins, are all in a greatly dilated state, their action, or propelling force on the blood, according to Dr. M., is diminished; and as the mass of the blood is diminished by the increased secretions, how can its momentum be increased when the power which moves it is also diminished? The doctor himself, with all

his sagacity, has been guilty of a palpable contradiction on this point; for he tells us in the same paragraph, that the momentum of the blood is both *reduced* and *increased* in the sweating stage of fever. The hot stage, he says, is put an end to by the evacuations which supervene in the sweating stage; "which, by diminishing the quantum of fluids, and removing irritating recrementitial particles, may *reduce* the mass, and *momentum*, and stimulus. The *momentum* of the circulation *is now very great, &c.*"

The author of the "Reply," has given us a cento of disingenuous quibbling on this observation in our review. "The action of a circular fibre, as Dr. M. correctly observes, tends to lessen its diameter, and, on the contrary, its *relaxation* must increase its diameter?" This sentence, says our shrewd theorist, contains two propositions: "1*st*, That the *action* of a fibre tends to shorten that fibre. 2*d*, That the *action* of a fibre must *increase* its length." This miserable perversion of our words and meaning, is scarcely worth an answer. Do we say that *relaxation* and *action* are convertible terms? If a circular fibre contract, and thereby shorten its diameter, common sense informs us that when it relaxes, that is, when the contraction ceases, its diameter must be elongated. When speaking of the action of the heart, which, by the by, is not composed of *circular* fibres, we adopted that side of the contested question in relation to its action, which alleges, that the dilatation of this organ is not a *relaxation*, but an active effort of its contractile structure. On this point, Dr. M. observes, "and he even pretends that the heart can spring open with wonderful force." We are not the only persons who pretend to think so. In this opinion we are supported by the authority of Bichat, Pechlin, and many other physiologists, of the highest reputation.

Dr. M. carps at our remark, "that increased action of the heart alone does not constitute increased action of this organ." If the testimony of Bichat and others, that the heart dilates by an *active effort*, is to be credited, then the action of this

organ, taken in its proper sense, consists of both the dilating and contracting efforts, as they succeed each other alternately.

For our belief that the heart is not passive in its dilatations, not a mere remission from action, but as much the effect of active powers of the heart, as its contraction, we quoted the experiments of Bichat. It is quite amusing to hear Dr. M's. objections to the evidence of this great physiologist. "If the reviewer," he says, "had given one sober thought to Bichat's article on the phenomenon of relaxation in organic muscles, he would have perceived that that great man was led, by his passion for analysis, in direct opposition to *reason* and *fact;*—that each, and every one of the six reasons advanced in proof of his *idle* doctrine, is alike destitute of foundation; and finally, that the whole error has arisen from a *careless* performance of a very common experiment." We will extract the six arguments of Bichat on this head: 1*st*, "When we lay bare a hollow muscle, the heart, the stomach, or the intestines, and empty it entirely of the substances that it contains, it contracts and dilates alternately, as when it is full, if we apply an external stimulant to it. 2*d*, If we empty by punctures, all the great vessels which go to the heart, or come from it, so as to evacuate it entirely, its alternate dilatations and contractions continue for some time. 3*d*, In order to judge comparatively of the degree of force of the contraction and dilatation, we can extract two hearts, nearly equal in size, from two living animals; place immediately the fingers of one hand into the auricles of the ventricles of the first, and grasp with the other hand the exterior of the second; you will feel that one makes as great an effort in dilating, as the other does in contracting. This fact already observed by Pechlin, is so *much the more remarkable, as the effort of dilatation is often greater than that of contraction.* I have even observed in *repeating this experiment*, that whatever effort we make with the hand, we cannot prevent the organ dilating. 4*th*, The alternate extension and contraction from which the vermicular motion of the intestines arises, is seen

during hunger, when we open the abdomen. *5th*, The hardness of the organic muscular texture, is as evident during dilatation, as during vacuity. *6th*, I have many times observed at the instant in which I irritated the heart with the point of a scalpel, that dilatation was the *first* effect of it, and that contraction was only consequent to it."

Let us now attend to the observations of our theorist, on the foregoing observations of Bichat. "It is most astonishing," he says, "that so acute a physiologist could have overlooked the real state of this case, (*i. e.* the third experiment on the heart,)—*that he should have mistaken for the diastole in the last mentioned heart, what was in reality the systole.*" A very modest declaration truly! Bichat tells us that he often repeated the experiment, and that he frequently found "the effort of the dilatation greater than that of contraction;" he must therefore have compared the dilatation with the contraction, and consequently distinguished them from each other. If he mistook the contraction for the dilatation, still, the fact of an active dilatation is substantiated, as the degree of effort experienced in either state was nearly equal. Again Bichat says, that "in irritating the heart with the point of a scalpel, he has often seen that dilatation was the *first* effect, and that contraction was only consequent to it." Does Dr. M. pretend to say that Bichat could not tell the difference between the dilatation and contraction of the heart when it lay before him? The observation of Dr. M., that Bichat was led by his *passion for analysis*, in direct *opposition to reason and fact*, is novel, and may be prodigiously wise, although to us it sounds very much like nonsense. How a passion for analysis should lead a man in opposition to *reason* and *fact*, we must leave to Dr. M's. "penetrative acumen," to determine.

In our review we say, "The principle of contraction is elastic power, &c. &c. This *elasticity is what Bichat calls the contractility of texture.*" Here our sapient theorist exclaims with great surprise: "What Bichat calls

contractility of texture!!" and then exultingly quotes from Bichat, as he imagines, to our utter discomfiture. The passage in our review is as follows: "It must be recollected that the arteries are not muscular tubes; that they do not possess the irritability and contractility peculiar to muscular structure; that, in short, *their principle of contraction is totally distinct from that of the heart; it being the consequence,* not of an excitation of muscular contractility, *but of an elastic power inherent in the arterial structure,* by virtue of which these vessels dilate or contract, according as they are distended by mechanical *force. This elasticity is what Bichat calls the contractility of texture.*" Now let us see whether Bichat will bear us out in this assertion.

Bichat divides the properties of the arterial tubes into two classes. 1st. Physical properties; 2d. Properties of texture. *Physical elasticity* constitutes the first class: and contractility of texture belongs to the second. These two properties are essentially distinct. Physical elasticity has nothing to do with the *contraction* of the arteries. "This elasticity, says Bichat, keeps the parietes *apart*, though they may be *empty*. It is to this elasticity of the arterial parietes that must be referred their recovering themselves when they have been flattened so as to obliterate the cavity. This elasticity requires for its exercise a previous *compression*, and is manifested by the sudden return of the parts to their natural state. Elasticity is purely a physical property." Of the contractility of texture, he says, "The contractility of texture takes place only when there is want of distention of the arterial parietes; that is, when the vessels cease to contain the blood which resists their contraction, or when they are *cut and afterwards left* to themselves. Contractility of texture has a permanent tendency to contraction; it is a property of vital matter *only*." Hence the contraction of the arteries, according to Bichat, depends solely on *contractility of texture,* and on the other hand, their power of recovering their diameter, when mechanically compressed, on their physical elasticity.

When, therefore, we say, as we do in the above paragraph, that "the principle of contraction (in the arteries,) is totally distinct from that of the heart, it being in consequence of an elastic power, *inherent* in the *arterial* structure, and which power Bichat calls the contractility of texture;" we quote Bichat correctly, and apply his term, according to its correct signification. The idea which we wished to express, and which we obviously did express, in the above paragraph from our review, is, that the principle of contraction in the arterial tubes is elasticity; which elasticity is *inherent* in the *arterial structure*, being the same power which Bichat calls the contractility of texture.

Dr. M. says, that when an artery is cut off, the "ring" is neither larger nor smaller than the *elastic coat* makes it. Let him read Parry's and Bichat's works attentively, and he will find, that what the former calls *tonicity*, and the latter *contractility of texture*, keeps the end of a cut artery, while alive, in a state *considerably more contracted* than it is when these powers cease, in consequence of the death of those animals, and when *physical elasticity* alone is in force; and that, consequently, it is incorrect to say, that "the ring is neither larger nor smaller than the *elastic* coat makes it."

In our review we stated, in conformity with the views of Bichat, which we believe to be correct, that the capillaries *dilate* and contract altogether independent of the heart and arteries. Against this position Dr. M. observes: "A circular fibre can only shorten and not lengthen the diameter of a vessel;" and hence a capillary vessel can have no power of dilatation in itself, but must depend for the degree of its dilatation, on the quantity of blood which the heart forces into it; "What then," he says, "becomes of the reviewer's surpassing assurance, that the dilatation of the capillaries is altogether independent of the heart?" If the want of proper knowledge, which this interrogation betrays in the doctor, can

put a *blush* in his countenance, he will bear a demonstration in his face, that the capillaries can and do dilate "altogether independent of the heart." We shall be glad to know from the doctor how the heart influences, or rather, determines the sudden dilatation of, and flow of blood to, the capillaries of the face in blushing. The following facts go to demonstrate that the doctor's dictum, in relation to elongation of a circular fibre, is not correct. "When we lay bare a hollow muscle, says Bichat, the heart, the stomach, or the intestines, and empty it entirely of the substances that it contains, it *contracts* and *dilates* alternately if we apply an external stimulant to it." What now becomes of the doctor's "surpassing assurance," in saying, that "a circular fibre can only contract, and not lengthen its diameter?" Again, a muscle laid bare, and extracted from the body, contracts, and afterwards *dilates*, without being drawn to its state of dilatation by any cause. In an amputation, we often see in the stump the end of the divided fibres alternately lengthen and contract. The iris too is an example of this kind of dilatation.

Dr. M's. assertion, that the theory of Bichat, in relation to the change in the capillaries which determines dilatation in them, is "reduced to the true doctrine of dilatation from *debility*, actual or relative," betrays an ignorance of the true import of Bichat's doctrine. By organic sensibility every organized vessel admits or rejects fluids according as it is able or not to support them. Thus, the lacteals absorb chyle only, because the qualities of this fluid are in accordance with the kind of sensibility with which these vessels are endowed; but they refuse other fluids, because such fluids are not in accordance with their sensibility. This specific, or organic sensibility of a capillary is capable of undergoing modifications, and the capacity of the vessel for admitting fluids is then also modified. Such a change, however, does not consist in *debility*. The vessel may be as strong after the change in its organic sensibility, as before it took place; it has only

acquired a new modification of sensibility which disposes it to admit either fluids, that it was not previously able to receive, or larger portions of that which it naturally conveys. The pylorus, when the food is first received into the stomach, contracts, because the food in that state is not in accordance with its organic sensibility. When the food has become digested, its new properties put it in a proper relation with the specific sensibility of the pylorus. This passage then opens and suffers the digested mass to pass through it. It would be absurd, however, to call this change, *debility*. If the food remain long in the stomach, without undergoing a proper change from the digestive process, it changes the organic sensibility of the pylorus, which then admits it in an imperfectly digested state. It is, therefore, not true, that a "change of organic sensibility, by which dilatation is produced, is reduced to the true doctrine of dilatation from debility."

Our theorist thinks it "surpassing assurance" in us, to say, that "the dilatation and contraction of the capillary vessels, depend on a principle altogether different from that which determines the same changes in the arterial tubes." It is pleasant enough, to hear the author of this splendid theory we are considering—call it "surpassing assurance," to maintain an opinion which Bordeu, Bichat, and other able physiologists, have considered as true. The doctor, however, is exerting himself to say a great many smart and severe things, and for which he is certainly to be excused, as it was very unpardonable in us to show that his theory, instead of being "the waters of Jordan," is in fact but a stagnant and very shallow pool. Dr. M. cannot help but smile at our "solemn annunciation, that the capillaries, which *act* by an inherent principle of contractility, are in every inflammation dilated, and, if you please, debilitated, while in fever, on the contrary, the arteries are enlarged, not by their own action, but by the larger quantity of blood forced into them by the heart." But, says the smiling theorist, "if dilatation follows weakness, why does it follow it; because, as before said, there is forcible

injection by the action of the heart." In the first place we may observe, that we do not admit, that the capillaries *are* debilitated in an inflamed part; our expression "and if you please debilitated," was merely conceding a point, for argument's sake, and which we did not intend as declarative of our own belief. We believe, with Hunter, and others, that the capillaries enlarge by "an act of dilatation," the opinion of Dr. M. to the contrary notwithstanding. But we must stop replying to the quibbling of our theorist:—We will only add a few more remarks on the absurdities into which his theory leads.

In the first place we are told, that the heart and arteries, act most violently in the cold stage of fever; and that they act with less violence in the hot stage; because, in the former the pulse is contracted, in the latter, it is expanded. Now, according to this idea, we must admit the absurdities, that the more vigorously the heart acts, the more feeble is the circulation; and on the contrary, the more feebly it acts, the greater is the velocity and momentum of the blood. We must also admit, that cold, bleeding, and syncope, excite the heart to more vigorous action, while, brandy, camphor, and opium, render its action more feeble. Now, although, we must admit all these, and a great many other ridiculous conclusions, if we adopt this theory; yet the author seems wroth, at our refusing to consider it as very rational and excellent. Dr. M. concludes by advising us to study the physiologists; this advice is certainly good, and we do not object to adopt it; as we would not, however, be outdone in offering friendly counsel, we beg leave to recommend to him, a very careful study of Reids' work on the principles of Common Sense.

E.

ANALECTA.

1. *Cholera Morbus of India.*—M. Moreau de Jonnes, who has been occupied during the last four or five years, in investigating the nature of this disease, makes the following remarks respecting it: That it has traversed an extent of country 1340 leagues North and South, and 1900 from East to West. *2d*, That it is not governed in its attack by individual predisposition; by age, sex, or temperament. *3d*, That it does not depend upon the state of the atmosphere, but is equally severe at all seasons. *4th*, It is not the effect of humidity, nor confined to low marshy situations, as it appears in the mountainous region of Nepaul, the highest spots in the isle of France, in the sands of Arabia, and the desarts of Diarbekir; consequently, it owes its origin to some other source than marsh miasmata. *5th*, Its long continuance (seven years) proves that it cannot depend upon a vitiated state of the air. 6th, That it is not consequent to peculiar diet, as the rice of Oude, or the fish of the Ganges, as many have fallen victims to it who have not used these, or similar articles of food. *7th*, It is not transported by currents of air, as it has been seen to spread in a direction contrary to that of the prevailing winds. *8th*, That this disease has no connexion with common cholera, but is a pestilential and contagious malady of a peculiar character.*

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2. *Hydrocephalus.*—Dr. Maxwell, of Dumfries, has made some observations on hydrocephalus, illustrative of the good effects of blood-letting. The following instance describes a practice which has been successful in a large number of cases. The patient was bled in the jugular vein, "till syncopy began to take place; a little negus was then given; when the pulse revived, the finger was removed from the orifice and the blood allowed to flow until the pulse could no longer be felt. The patient had at this time no appearance of life, and continued without the least symptom of animation for ten minutes, when he began to revive gradually, and in the evening more favourable symptoms appeared. His mind was remarkably improved, as well as his physical powers, being now able to articulate, although indistinctly, and to tell the hour on a watch. He had a tolerable night's rest, having taken frequently some water-gruel and beef-tea. During the following day there was but little improvement, on the third day the bleeding was repeated in the left jugular vein, and a complete recovery followed."

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3. *Opium in Inflammatory Diseases.*—Dr. John Armstrong, author of a late Essay on Typhus, has made some valuable observations on the use of opium in inflammatory diseases. He ascribes the neglect of this remedy, in a great measure, to a general belief that it is merely a stimulant.

In peritonitis he has used it with great success, in combination with calomel, premising its exhibition by copious blood-letting. He gives it in doses of from two to four grains, with about as much calomel, and if this produces no good effect in the course of five or six hours, it should be repeated. He observes that he has seldom found it necessary to use more than two bleedings, and two doses of calomel and opium in any case; a third process, however, is to be gone through after an interval of six hours from the second, if necessary.

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4. *Hydrogen.*—A curious fact in relation to hydrogen gas, has lately been observed. If a stream of this gas is directed upon powdered platinum, it is inflamed. To insure the success of this experiment, the platinum should be slightly heated,—perhaps to divest it of accidental moisture.

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* We will give some observations on this interesting subject in our next number.—*Ed.*

5. *Experiments on Variolous Inoculation.* By William Maxwell, M.D., Dumfries.—In 1801, the matter of small-pox was kept for a fortnight and inserted; the disease exhibited the common symptoms of variola. "Matter taken from this subject was used at the end of a month, without any deviation from the usual symptoms. Inoculation was again repeated at the end of six weeks, as in the foregoing experiment, without any sensible alteration in the result. In experiment fourth, matter was taken from the last case and kept two months before insertion; when the patients, two in number, appeared to suffer less from fever; the pustules were earlier than usual; I thought they scabbed sooner, and had a narrower inflamed margin; in short, that the disease began to acquire a sensible degree of mildness. In experiment fifth, at the end of ten weeks, matter taken from one of the subjects in last experiment was used in several children; in some of them it failed to produce infection; where it succeeded, the disease appeared less active: the pustules began to scab on the sixth day. Experiment sixth was made with matter taken from one of the subjects of the preceding one; it was inserted at the end of three months; the eruption appeared on the fifth day with little or no fever; the scabs fell off sooner than in the last experiment, but in this there might have been some inaccuracy, as I had it only from report. Experiment seventh was made at the end of fourteen months; two subjects only out of nine who were inoculated, were infected; the eruption appearing on the end of the fourth or beginning of the fifth day, without fever or indisposition: the pustules were smaller, more prominent, like the small end of an egg, and almost entirely free from inflamed margin: the scab, of a foliated structure, fell off about the eighth day, without leaving any pittings. Four sets of children were now inoculated with this matter, applying it directly from the pustules: the disease in all respects resembled that in last experiment, the children playing about in a state of perfect health.

Although the periods of inoculation are said to have been at the ends of weeks or months, they were sometimes a few days sooner or later, but not varying more than four. This irregularity was occasioned by attention to my own convenience, as most of the experiments were done in or near the village of Newabbey, a distance of seven miles from Dumfries. I have not heard that any of the subjects of the above inoculation have been infected by either small pox or chicken-pox, although they have both been in the village, and no means were taken to prevent communication with the infected. Here the author's investigation ceased, giving way to vaccination, which extended rapidly."

These experiments go far to show that the varioloid disease and small-pox, if any doubt could now remain on the subject, are one disease. If it be true, as proved by the experiments of Dr. Carpenter some years ago in Philadelphia, that the vaccine pustules can be produced by the inoculation of the cow with the small-pox, we have the whole enigma solved of the success of the Jennerian discovery. The different degrees of power of the variolous contagion will explain the existence of chicken-pox, of the varioloid disease and variola, from the agency of one poison alone: Future observations must determine how far, like measles, to which some of the varieties of the varioloid disease approximate, it may originate from causes probably connected with the state of the atmosphere.

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6. M. Graefe's *Case of Lithotomy.*—The operation was undertaken under very unfavourable circumstances in a countryman aged thirty-eight, who had suffered from calculous symptoms nearly thirty-five years, and was almost exhausted by pain and misery. That the stone was large could easily be seen beforehand, but after the bladder was opened its size was found to exceed all expectation. So great was it that the forceps, however carefully applied, slipped off, or sunk into the concretion, or even bent at the handles; but all in vain. Professor Graefe at last succeeded in extracting it, by in-

troducing three lithotomy scoops into the bladder, and using them (with the aid of an assistant) as levers. Immediately after its extraction, the stone weighed twenty-one ounces and nearly four drachms. Its length was four inches, three lines; its breadth three inches, nine lines; its thickness two inches, ten lines; its longest circumference eleven inches, nine lines; its shortest nine inches, ten lines—all Paris measure. The patient was at first much relieved, but unfavourable symptoms soon appeared, and he died on the 11th day. The section showed that the wound in the bladder had contracted to the size of two inches; that the bladder had not been lacerated, and that it was not even inflamed. From the immense size of the stone which had completely filled the bladder, and therefore constantly obstructed the passage of urine, the ureters were dilated as high up as the kidneys, into large bags which had contained the urine.—*Anderson's Quarterly Journal, July*, 1824.

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7. *Of the Section of the Fifth Pair of Nerves.*—M. Magendie has recently communicated to the Academy of Sciences the result of his researches on the functions of the fifth pair of nerves. He found, after dividing this pair of nerves, that several senses were suspended. Believing that this arose merely from the injury occasioned by the operation, it did not at first excite his attention; but remarking that the animals thus operated upon did not recover their senses, he directed his inquiry to these points. The same results attended his experiments on the sense of smelling: in the memoir presented to the Institute, he also states, that after the division of the fifth pair of nerves, the eyes are deprived of their sensibility, and of a considerable share of their appropriate functions—sight is nearly lost: they quickly become the seat of inflammation, and waste rapidly. The tongue also loses its sensibility; and sapid bodies seem to produce no sensation on the anterior part of the organ of taste.—*Lond. Med. Rep. July*, 1824.

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8. *Strychnine.*—In the nux vomica, the bean of St. Ignatius, and the celebrated poison of Java, there is a peculiar vegetable alkali termed strychnine, one of the most virulent poisons in nature, as we have before said. It is soluble in water slightly acidulated, and so intensely bitter, that it is decidedly perceptible when diluted in *sixty thousand* times its own weight of water. The sixteenth part of a grain paralyses small dogs in four or five minutes, and kills them in less than half an hour. Its action is peculiar and uniform, paralysing invariably the hinder extremities.—*Lond. Medico-Chirurg. Rev. June*, 1824.

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9 *Physiological Action of the Salts of Bark.*—A considerable number of experiments have now been made to determine the medical or physiological effects of these medicines, and it is well ascertained, that they possess the properties of the cinchonas, and consequently that they may, in almost all cases, be substituted for them. It is, in the first place, desirable to ascertain the precise dose of all active remedies, and this is done better with the alkalis in question, than with the bark, from which they are obtained, and in which they exist in different quantities according to the state of the bark. In the second place, it is often, we might say always, desirable to administer medicines in a small volume, and agreeable form. In these respects the salts possess great advantages. Chemistry, therefore, has done much service to medicine, by separating the active principles of vegetables from the inert substances with which they are combined in a natural state.

The sulphates of quinine and cinchonine may be given in doses varying from one to ten grains in the twenty-four hours. Some have carried the dose much higher, but this is seldom necessary. Large doses, indeed, have produced inconvenience, if not danger, giving rise to strong cerebral excitement. A wine or tincture of the sulphate of quinine may be made thus:

—Sulphate of quinine, 10 grains; Madeira wine, two pints. The tincture—sulphate of quinine, five grains; alcohol, one ounce.—*Lond. Medico-Chirurg. Rev. June*, 1824.

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10. *Effect of Colchicum.*—"Mrs. ———, aged forty, after frequently suffering from gout, requested her medical man to give her the colchicum in a very severe fit.

"She took ʒiiss. of a tincture made by infusing ʒiv. of the root in ʒviij. of proof spirit for three days, the mixture being kept at nearly 100° of temperature. This was given in the morning of Dec. 5. In the evening it had produced no effect, except slight qualmishness. Calomel gr. iij. opii gr. i. was ordered at bed-time, and a purging draught for the morning. However, in the night, vomiting and purging commenced, and continued all the next day, in spite of effervescing volatile saline draughts with opium; so that in the evening of the 6th, oppii gr. i. camphor gr. iii. were given and repeated in two hours.

"On the 7th, from accident, she was not seen till three *p. m.* when she was found in the collapse preceding death. The gout had previously gradually subsided. It was stated that she became faint at two o'clock *p. m.* and not till then were her friends alarmed. By opium and spirits, warmth was reinduced upon the extremities, and a feeling of greater comfort produced; but the pulse never completely recovered, although the sickness was completely subdued; so that at ten *p. m.* she fell into an *apoplectic kind of sleep*, which terminated in death before morning."—*Lond. Medico-Chirur. Rev. June*, 1824.

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11. *Intestinal Tympanites.*—Dr. Levrat, Physician of the Hotel Dieu of Lyons, has related a curious case of this kind which was cured by puncturing the intestine, and setting free its gaseous contents. The patient was Madam Lepin, to whom he was called on the twenty-fifth day after a painful delivery, in which the fourchette was torn. He found the patient apparently dying; with facies hippocratica, small creeping pulse, cold extremities, and tumefied abdomen, which was very painful on pressure. Cordials, anodynes, and emollients were first prescribed, and removed the most urgent symptoms; but intestinal tympanites remained, and continued in spite of various medicines to increase to an enormous extent. The convolusions of the bowels could be distinctly traced, and the air could be heard rushing from one knuckle of intestine to another. Emaciation had made great progress. Dr. L. determined on an operation. A very delicate trochar was constructed, not much larger than a darning needle, and about two inches in length. This was pushed through the parietes of the abdomen, on the right side, midway between the navel and the spine of the ilium, and into a projecting roll of inflated intestine. Immediately the stilet was removed, a quantity of gas rushed out through the canula, and the belly experienced a great reduction in size. The patient passed a good night, and next day a large quantity of hardened scybala came away, with infinite relief to the lady. From this time she went on well, and rapidly recovered. We forgot to mention that the canula (corked) was left in for some hours, and then some more gas drawn off.

This operation has been practised before, for tympanites, but not, we believe with success—probably because the wound in the parietes of the belly and in the intestine was made with too large an instrument, in consequence of which, gas or other matters may have escaped from the intestinal tube into the general cavity of the peritoneum, and there excited inflammation. In Dr. Levrat's operation, the instrument was nearly as fine as that used in acupuncture, and consequently the wound in the intestine was so small as not to permit the escape of any intestinal contents into the

cavity of the peritoneum, when the trochar was withdrawn.—*Bulletin des Sciencis.*

12. *Antimony in the Phlegmasiæ.*—A mystery and doubt still hang ovee the Italian mode of treating the phlegmasiæ by large doses of the tartratr of antimony. Dr. Fontaneilles has recently published his testimony respecting this litigated question, in the Archives Generales for February last, from which we shall abridge some particulars. We need hardly remark that the employment of tartar emetic is no new practice in the phlegmasiæ—but it was only considered as an auxiliary to blood-letting and other evacuations. The peculiarities of the Italian practice, and especially that of Rassori, consist, 1mo. in treating pneumonia, from beginning to end, with this medicine—2do. in making it the principal, or almost the sole remedy—3tio. to thus reduce, or rather discard blood-letting.—4to. to exhibit the medicine in doses that would appear frightful to former practitioners—that is, to the amount of a scruple, or even some drachms in the twenty-four hours, and that without producing either vomiting or violent purgation.

The first accounts of this practice staggered the belief of physicians, who insisted that some fallacy must have crept into the reports. It was said that the medicine could not have been of its proper strength, or that the patients could not have swallowed the doses that were prescribed. Both these doubts have been cleared away, if we can credit the assertions of Dr. Fontaneilles (Physician of the Military Hospital of Milan) who has given the medicine to be analysed and experimented on by those who were competent to the task of both. Many physicians scrutinized the exhibition of the remedy in the doses described—without being able to detect any fallacy in the practice.

Our author observes that the capability of sustaining such large doses of emetic tartar depends entirely on a certain morbid state of the system—for when this morbid condition is removed, the power of taking the remedy in large doses with impunity or advantage immediately ceases also. This, he insists, is the true and only solution of the enigma; and this indeed is most indubitably the cause, as we have hinted in a former number of this journal. This morbid condition, or diathesis, which bears the reception of such large doses of the antimony, varies in different cases of peripneumony, (for it is of peripneumony which our author treats in this paper) and in different stages of the same case. Thus, this diathesis is less prominent in the beginning and in the decline of the inflammation—and most powerful at the acmé of the disease. The doses of the medicine must be regulated according to these epochs, otherwise their common effects will be produced. In general our author commences with not less than twelve grains in the day, and the same quantity in the course of the night. But when he finds that the pneumonia has made progress, he gives from a scruple to half a drachm in each of the above periods, gradually augmenting the quantity, according to the violence of the disease. Sometimes, but merely as exceptions to general rules, the dose of the medicine exceeds the ratio of the diathesis, and then vomiting is the result. At other times, the violence of the symptoms, as the cough, pain, dyspnœa, &c. will abate, while the force of the diathesis remains undiminished—as known by the capability of bearing the large doses still continuing. We must not therefore diminish the dose with the diminution of the symptoms—*unless we find the power of bearing the medicine also diminish.* Again, some of the troublesome symptoms may persist, notwithstanding that the diathesis is declining. "Thus the breathing may become more short or laborious, or delirium and tendency to coma may supervene—and, if, in these cases, we find a diminished aptitude in the constitution to bear the large doses of antimony, we may conclude that organic changes are taking place, which will set at nought the resources of our art."

The cessation of the diathesis is soon proved by the patient not being able to bear the smallest quantity of the tartar emetic, without loathing and disgust, although previously swallowing large quantities without the smallest inconvenience, but, on the contrary, benefit. Although our author looks upon emetic tartar as a direct sedative, and in this respect similar to digitalis, yet he observes that its exhibition, in full doses, is not attended with the same bad effects as sometimes attend the latter medicine. He has never seen the pulse reduced below fifty in the minute by antimony, and but rarely any irregularity in the action of the heart.

Our author has been asked why he does not abandon blood-letting altogether, since he has found so effectual a remedy for pneumonia in emetic tartar? His answer to this question is, we think, a very wise one. In many cases, says he, the pneumonia runs its course so rapidly, that the structure of the lungs is menaced with destruction, which circumstance induces to put in force, *simultaneously*, different means of reducing the phlogistic diathesis. On the other hand, he deprecates the subtraction of so many pounds of blood in this disease, when venesection may be so powerfully aided and greatly abridged by the use of antimony. He properly impresses it on practitioners that blood-letting is never to be dispensed with in this class of maladies, especially when they are severe, since the effects of this remedy are prompt and certain, where time is so precious; whereas, it requires a certain period to elapse before the antimony produces its operation in checking the inflammatory process.—*Med. Chirur. Rev. June*, 1824.

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13. *Belladonna as a preventive.*—Among the very few novelties in the department of Therapeutics we have to introduce to our readers, is the system of Dr. Hahnemann, under the name of *homœopathia*, which is used to designate the method of treating diseases by means of active medicines in doses of extreme minuteness. The fundamental principle of this theory is, that different complaints are cured by means which produce symptoms analogous to those of the disease: and that the remedy is appropriate in proportion to the strictness of the analogy between its effects and the disease it is intended to cure. Thus, a chronic disease may be arrested by an acute one, and vomiting stopped by an emetic: in short, the principle is *similia similibus curantur*. Dr. Hahnemann, proceeding upon this idea, argues that, as an atom of vaccine virus is sufficient to give protection against small-pox,—or an atom of any other morbid poison, as syphilis, is capable of giving rise to a disease similar to that in which itself has originated,—so atomic doses of active medicines, provided they be the proper contra-stimulus to the disease, are regarded as preferable to the same medicines in larger quantities. There appears much of hypothetical assumption in these doctrines, so far as we have yet been made acquainted with them; but numerous plausible cases are cited,* and we are promised further details in a system of Materia Medica, by Hahnemann.

We have alluded to the views of M. Hahnemann, however, chiefly for the purpose of introducing one of the most important illustrations of his doctrines,—one which, rejected in France, and almost unknown in this country, is nevertheless regarded by many in Germany as second in importance to the discovery of vaccination alone: we allude to the alledged efficacy of *belladonna as a preventive of scarlatina.* We are told that the author of this system, having observed that the drug in question, when taken in very small doses, produced symptoms analogous to those of scarlatina, was hence led to the conclusion that it was the proper antidote to the disease; a conjecture which at first was regarded, even in Germany, as too improbable to merit serious attention. But, lest our readers should still think thus

* By Dr. Wiedmann in Hufeland's *Journal der Practisch Heilkunde*, 11 cahier, 1823.

of Dr. Hahnemann, and his claims to be regarded as a second Jenner, we shall lay before them the opinion of Professor Koreff, in a letter to M. Laennec; an opinion which, he informs us, is founded on the observations of sixteen years.*

"Observation clearly proves," says he, "that the belladonna, taken for some time, either in powder or in extract, produces, especially in infants, a redness of the skin, which is sometimes transient, but at others more durable; dryness of the mouth, with a sense of heat in the throat; dilatation of the pupil; anxiety; and occasionally swelling of the submaxillary glands: symptoms having a great resemblance to those which accompany the eruption of scarlatina. The effect of the belladonna has also this in common with scarlatina, that neither of them produce the redness of the skin invariably, whilst the symptoms about the throat are always present. I confess to you, however, that all these analogies did not appear to me sufficiently strong to persuade me that in this plant was really to be found a preservative against scarlatina, similar to that which the cow-pock affords against variola. It was not till I had received the authority of the celebrated Soemmering, who informed me that he had obtained the most satisfactory results with it when the disease raged epidemically, that I determined to employ it. This malady, accompanied by the most unfavourable symptoms, and having entirely changed its usual character, was at that time producing ravages almost as fatal as the contagious typhus. I then, for the first time, had the happiness to protect from this dreadful contagion almost all those who took the belladonna with a little perseverance, and of these there were many thousands. Since that time I have never lost sight of this discovery, which becomes the more valuable as the scarlatina has increased during the last thirty years, both in violence and extent, in many countries; and I have always found the same effects in different climates, and in epidemics of opposite characters. Many other physicians have equally confirmed the preventive powers of this plant, and the German Journals are daily filled with proofs of a benefit which, with respect to some countries, equals that of vaccination. In France, the capital and the provinces of which appear less subject to these fatal epidemics than Germany, Swisserland, the Tyrol, Poland, and the north in general, less attention has been given to this discovery, and it has been rejected,—it must be said too lightly, and without any sufficient examination, as may be seen in the article *Belladonna* in the Dictionnaire des Sciences Medicales. I only remember a single observation on this important subject, by Dr. Meglin,† who gives an account of a trial which he gave to this preservative during an epidemic of scarlatina at Colmar, and which confirms all the assertions of the German physicians. The absence of present danger is, perhaps, the cause of this indifference towards a discovery, which, important in itself, might also be fruitful in results applicable to other diseases. At present, however, I shall confine myself to an account of the results which have been ascertained, (by repeated observations, and by a great number of individuals placed in very different circumstances,) without incurring the reproach of having proceeded in a manner not sufficiently rigorous.

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"The powder mixed with sugar, or the extract made very carefully from the juice of the recent plant, are employed after the following formulæ:—Extract of belladonna, three grains, dissolved in an ounce of cinnamon water. Powder, or root of belladonna, two grains, mixed with ten drachms of white sugar, divided into sixty doses. From half a dose to a whole one is

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* *Sur l' Emploi de la Belladonne contre la Contagion de la Scarlatine: Note communiquee a* M. le Prof. Laennec, *par le* Prof. Koreff.—(Bulletin des Sciences Medicales, Avril, 1824.)

† *Nouveau Journal de Medicine*, 1821.

given to a child from six months to two years old, four times a-day; to children from three to six years old, from a dose to one and a half; to those from six to nine, two to two and a half; to those from ten to twelve, three to four and a half. Of the solution, a drop is given for every year of the child's age, once a-day, and fasting. Observation has shown that, when the epidemic is very fatal, or the intercourse with the patients very frequent and intimate, it is prudent to increase the dose a little. It has not yet been possible to determine, in a satisfactory manner, the length of time which is necessary to eradicate, by this remedy, the susceptibility of the contagion. Every thing leads us to believe that the remedy, if used during a time too short to ward off the contagion, moderates very much the malignity of the disease. We know for certain that the remedy does not permanently overcome the disposition to scarlatina; and it is necessary to resume its use on every recurrence of an epidemic. We have always observed that the most intimate communication with the sick does not produce the disease, provided the medicine has been employed eight or nine times previous to being exposed to the contagion, and continued up to the period of desquamation; a circumstance very important to nurses. It appears more certain to begin with rather strong doses, in order to guard against the first impression of the contagion, and to diminish the quantity after a few days. No sensible effect has been observed to follow the continued use of this small quantity of belladonna. Up to the present time, neither season nor locality, nor any other circumstance, has appeared to diminish the preservative effect of this plant."

After remarking that the belladonna does not exercise the same power over miliary scarlatina, he proceeds:—"Do not believe, my learned colleague, that these results have been too lightly deduced, or from a small number of individuals, or from epidemics of little violence. It is from entire provinces,—from cities affected with this terrible scourge,—from epidemics the most fatal, in all seasons, and in localities the most diversified,—on individuals of every age and of every condition, that observations have been made with the greatest accuracy, and have led to the above results.

Belladonna has been long used by Chaussier in cases of rigidity of the os uteri, in the following manner:—Rather more than two drachms (*huit grammes*) of the prepared extract are softened with an equal quantity of distilled water, and incorporated by trituration with an ounce of hog's-lard or simple cerate. In order to secure all the benefit which this means is capable of affording, it is necessary that this preparation be applied directly to the orifice of the uterus. To effect this, he has contrived a small syringe, which in place of having a pipe, is rounded at its extremity, and has an opening large enough to admit the point of the little finger. The piston is pulled back a little, then about the size of a small nut of the ointment is introduced at the upper orifice; the syringe is directed by the finger to the mouth of the uterus, the piston is pushed home, and the ointment forced out; being of a soft consistence, it liquifies and spreads over the parts. At the end of thirty, or at most forty, minutes, the os uteri becomes soft and relaxed to such an extent, as no longer to present any resistance to the efforts to dilate it, or to the contractions of the uterus itself. This method was extensively employed by the late Madame Lachapelle, and is likewise commended by M. Legrand, the present midwife in chief at the Maison d'Accouchement.*—*Lond. Med. and Phys. Journal, July*, 1824.

14. *Colchicum Antumnale in Gout.*—Within a few years this remedy for gout obtained great reputation, and, indeed, was spoken of as new remedies generally are, in the most enthusiastic terms. Dr. Hall, of Philadelphia, used it in one case, in which the disease was translated from the foot

* *Journal Universal des Sciences Medicales*, 96 cahier.

to the brain, and it was only by means of excessive blood-letting, cathartics, and enemata, that he was able, in the course of forty-eight hours, to rouse the patient from the apoplectic state which threatened his life. As this metastasis was attributed to the colchicum, no more of this substance was prescribed, but a perfect cure was subsequently effected, by confining the patient generally to animal food, prohibiting all acid and fermented drinks, and using active exercise, and cold bathing. In addition to this treatment, a few cathartics and bleedings occasionally were necessary. We are induced to mention this case, as we find that the London Medico-Chirurgical Review contains the details of another, in which also the colchicum autumnale appears to have produced a determination to the brain. Dr. Hall has also witnessed a case, in the state of Georgia, in which a single dose of the vinous tincture of colchicum produced a dizziness and stupor, which, had it not been removed by immediate venesection, would, very probably, have run on to perfect apoplexy.

These cases indicate, that not only colchicum, but all other agents capable of arresting, suddenly, a paroxysm of gout, should, if used at all, be applied with great caution, and never in circumstances where the physician is not immediately within call of the patient.

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15. *Distortion of the Spine.*—Dr. Macartney, of Dublin, in a paper lately published in the Lon. Med. and Phys. Journal, observes, that distortion of the spinal column may arise from three causes. The first is produced by an ulceration of the bodies of the vertebræ, which induces an abrupt or sudden curvature. The second originates in a deficiency of earthy matter in the bones, and is curved into flexures, instead of being angular like the former. The third cause of distorted spine, which he states, is *a feebleness of those muscles which are employed in maintaining the erect position of the body.* The tendency of a weakened state of the muscles to augment the more serious kinds of curvature, and to induce, even without a softened condition of the bones, an evident distortion of the back, does not appear to him to have received sufficient consideration from medical men. Conformably with this view, he thinks that all confinement of the trunk of the body is not only calculated to produce the disease at first, by fatiguing and debilitating the muscles, but in a more especial manner, to confirm it after it has actually commenced. When it becomes necessary to relieve the spine from the weight of the superior parts of the body and the head, until by proper medical treatment the strength be sufficiently restored, he prefers the mode of resting on a flat surface, or, what is more comfortable, a gently-inclined plane, to any species of machine or bandage that has been invented. He thinks, however, that the effects of the recumbent posture are not always understood, and are sometimes over-rated. There appears to be no necessity for enjoining rest, as is usually done; on the contrary, the patient should rather be encouraged to exercise the spinal muscles by rolling about, and moving the arms and legs at pleasure: for which purpose, a large platform, stuffed with wool, or a matrass, or where children can have their toys, and play with their companions, is preferable to a sofa or plane board, which are most frequently employed.

The recumbent position is sometimes resorted to as the sole means of curing or preventing curved spine. In several celebrated boarding schools in London, it is used indiscriminately for all species and degrees of distortion: and, in some of the seminaries, all the girls are condemned to get their lessons during so many hours every day, in the surpine posture, with the view of preventing the occurrence of deformity. It appears to him more than unnecessary to confine healthy girls to the recumbent position: yet too much caution cannot be used to prevent children leaning uniformly to one side, in the act of writing, drawing, &c., more especially if they be weakly, or if they seem to prefer that particular position.

The advantages from exercising the spinal muscles in various positions of the body, should never be lost sight of; both as the means of preventing and of curing curvature, when the erect position can be safely sustained. In some cases, where the curvature has been slight, the persons very young, and the general health good, he has succeeded in restoring the natural form, merely by liberating the body from all constraint of stays or monitors, and permitting as much exercise to be taken as could be borne without fatigue.

Balancing any object, however light, on the head, has great efficacy in rectifying the shape of the spine. It is also readily adopted, as a pastime, by young people. In some instances of spinal curvature, where there was not a softened state of the bone, he has tried, with the best effects, exerting the muscles of the back, by the carriage of a bag of sand on the head; employing, at first, short periods of time for the exercise, and allowing rest in the intervals, in the recumbent posture; and, as the strength increased, repeating the exertion more frequently, and continuing it longer. He has every reason to believe that this treatment, if employed with proper caution, will be always useful in cases of curvature from weakness; and, if practised with assiduity during early youth, he is persuaded would be the best means of preventing distortion.

The means which are most beneficial, where merely the shoulders are round and prominent, are skippings with a rope, and the use of the dumb-bell; particularly in the action of carrying them backwards, which exerts the flat muscles that lie between the shoulder-blades and spine.

He concludes by observing, that in all cases where a curvature of the spine, from a softened state of the bones, is apprehended, or has taken place, every endeavour must be made to improve the health and strength, by a pure air, tonic medicines, and the cold bath; without which, machinery and rest will be useless, and exercise hurtful.

Some situations are much more productive of a disposition to curved spine than others. In certain districts of England and Holland, it is found to prevail. In the neighbourhood of Leyden, he has observed great deformity of the spine to be almost universal with the women, and to exist even frequently in men. Damp and low situations would seem to have a peculiar tendency to induce a softened state of the bones.

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13. The *pulvis antimonialis* of the London Pharmacopœia, was made the subject of experiment by Dr. Elliotson some time ago, with a view of ascertaining its real effects and proper doses. We are inclined to think his observations have not met with the notice they deserve,—at least, they have not had any influence with those to whom we are indebted for the New Pharmacopœia. Dr. F. Hawkins, apparently ignorant of Dr. E's. remarks upon this subject, has published a paper,* in which he has arrived at similar conclusions: indeed, he asserts that the effects attributed to this medicine, are not to be produced by ten times the usual dose; and he informs us, that he now begins by exhibiting half a drachm at once. One case is mentioned, in which two scruples were given twice a-day, for several weeks: "no function was in any degree affected by it." In another instance, so much as one drachm was exhibited night and morning, without any sensible effect. It is proper to remark, that Dr. H. procured some pulvis antimonialis from Apothecary's Hall, for the purpose of experiment.

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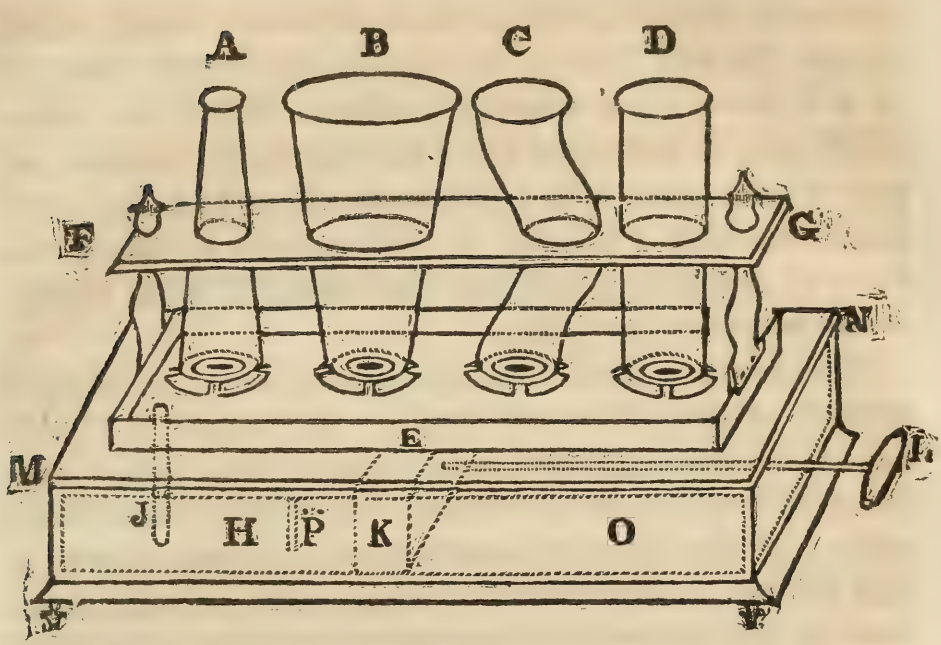
* *On the Dose of the Pulvis Antimonialis.* By F. Hawkins, M. D. &c.—(Edinb. Med. and Surg. Journal, April, 1824.)



A New Hydrostatical Instrument, by JACOB GREEN, Professor of Chemistry, &c.—A number of instruments have been contrived to illustrate the important principle in hydrostatics, that the pressure of fluids on the bottoms of the vessels which contain them, bears no proportion to the quantity of the fluid, but depends entirely on the shape of the vessel. At the first glance, it seems contradictory that the pressure of water, or any other fluid, on the base of a vessel shaped like a cone or an inverted funnel, should be so great as it would be, were that vessel equally wide from the bottom to the top, and which consequently would hold twice as much of the fluids.

The apparatus of Paschal, for the purpose of demonstrating the hydrostatical paradox, that a small quantity of a fluid may be made to counterbalance any quantity of the same fluid, is the instrument commonly in use; but I have always experienced great inconvenience with it before a class, from the sudden dashing out and splashing of the water from the stiffness of the valves, and other circumstances connected with the counterpoising weights. I propose, therefore, as a sub-

stitute, the following instrument, in which the vessels, which contain the fluid have a mercurial valve in their bases.—A B C D are glass vessels of different figures and capacities, open at both ends, and whose bases and altitudes are precisely the same: E is a wooden trough, an inch and a-half deep, containing a quantity of mercury, and in which the bases of the glass vessels are immersed. F G is a little frame to support the vessels.

As mercury is about fourteen times as heavy as an equal quantity of water, one inch deep of that metal in E will support a column of water about fourteen inches high, in the vessel D, which is of equal diameter throughout, and if more water than the above quantity should be poured into D, it will run out at the bottom. The application of this instrument to the hydrostatical principles which it is intended to illustrate, will readily be made by every one acquainted with the subject.

As the mercury used in this experiment would collect on its surface dust and other impurities, if suffered to remain in the trough E, there is a little cistern H, in the base of the instrument, where it may be preserved; into this cistern it is conveyed after experimenting, by the tube J, open at both ends, and which reaches from the bottom of the trough E nearly to the bottom of the cistern H. At the upper part of the side of the cistern marked P, there are two or three small holes bored, which form a communication from D to H.—K and L is a piston for forcing the mercury from H into E when required. The manner in which this is done will be easily understood by referring to the figure.

*** The Editors owe an apology to the Subscribers to this Journal, for the delay which has unavoidably been experienced in issuing the present number. As it proved impossible to complete it precisely at the termination of the last quarter, they thought it best to commence the year with the last half volume.

THE

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ART. I.—*Remarks upon Morbus Oryzens of India.* By ROBERT TYTLER, M. D. &c. 8vo. pp. 299.

Observations of M. MOREAU DE JONNES *on the Cholera of India.*

IN the London Medical and Physical Journal, for August, 1824, (No. 306.) there is an article of medical intelligence containing the result of *M. Moreau De Jonnes'* investigation of the nature of the *Cholera Morbus* of India. We regret that we have not seen this gentleman's essay, and that we are, consequently, ignorant of the arguments by which he supports some of the conclusions at which he has arrived, as set forth in the article above mentioned. Depending, however, upon the candour of the editors of the Medical and Physical Journal for a fair exposition of M. de Jonnes' views on this subject, we venture to inquire into them. We are not stimulated to this task by a mere spirit of criticism, nor by any desire to expose a fault in a work which, for ought we know, may contain much instructive matter; and, judging from our own experience, must have cost the author much in the articles of labour, privation, and danger. On the contrary, although we are

inclined to differ from M. de Jonnes on one or two important points, we are gratified to find that a disease so violent in its character, apparently so disposed to spread its empire through populous regions, and, consequently, threatening to become so fatal to our species, has been made a subject of regular investigation.

Another writer on the Cholera Morbus of India has also some claim to our attention, at least, for the novelty of his doctrine, and for the industry which he has displayed in loading many pages with its exposition. We allude to Dr. Tytler, who, during his residence in India, has published several pamphlets containing narratives of the appearance, progress, and effects of the disease in question, in various places within the East India company's dominion.

This gentleman is the author of an opinion respecting the cause of the Cholera Morbus, which we will introduce in the course of the present article.

According to Dr. Tytler, the disease made its appearance in the "*immense tract*" of country, constituting the district of Jessore,* in 1817, since which time it has become so extensively prevalent throughout India and its neighbouring islands, and even in the eastern and southern borders of Europe, that it may well be considered as one of the most interesting subjects of medical inquiry of the present time. The investigation of the cause of the oriental cholera presents many difficulties, on account of the suddenness of its attack, the violence of its symptoms, the rapidity with which it hurries its victims to the grave, and, lastly, the great extent of its prevalence in regions including such varieties of soil and climate as to forbid us to seek a local cause to which epidemics may usually be traced. The progress of the generality of diseases affords the practitioner sufficient time to make his observations, to correct his errors, and to trace the character of the morbid action in the symptoms which are successively

* 130 miles in length, and 80 in breadth.

presented. Not so, however, with this dreadful scourge of the East. It frequently happens that a continuance of this disease for from six to twelve hours, is sufficient to extinguish life. We have seen numerous instances of workmen beginning their daily task in all the plenitude of health and cheerfulness, and without any premonitory symptom, attacked with excessive vomiting and purging, which exhausted them in a few hours. Hence, there is but little time for the intervention of medicine, and none at all for a deliberate diagnosis.

Another cause of the great fatality of this disease, is the insuperable prejudice of the ignorant natives against the use of medicine from the hand of a foreigner. Some remarkable examples have occurred, in which the wretched sufferers preferred the certainty of death to the danger of offending against their religion and *losing cast*, by swallowing any thing that had been touched by a christian. On one occasion, as we were walking on the bank of the river opposite to Calcutta, we met a native who had just been seized with violent symptoms of cholera. Medicine was immediately offered to him, but he refused it. The danger of his situation was represented to him, still the proffered assistance was urged in vain—he professed to be perfectly conscious of his desperate condition, but he dared not take medicine from a white man. He was carried by his comrades into the shade of a neighbouring grove, and abandoned to his inevitable destiny, for they had learned to resign all hope of repelling an attack of this formidable enemy.

With regard to the remote cause of the cholera of India, Dr. Tytler prides himself in having traced it, in Jessore, where he first saw it, to the use of some rice which had been disseminated through that district; and while the disease was confined to one place, and to a few cases, in each of which the patient had indulged in the use of this article, the Doctor's opinion was at least plausible; especially if he accused this particular crop of *Ouze rice* of possessing deleterious properties. But as the disease continued from year to year, and

spread in every direction, long after this deleterious crop had been consumed, some other cause must be sought. The doctor pears, in his subsequent essays, to attribute it, therefore, to the use of *new* rice generally. He supports this opinion by relating many instances in which his patients had eaten new rice, and we are by no means disposed to doubt the fact that they had partaken, and largely too, of this diet; for we well know the propensity of the Hindoo for a good mess of fresh rice. But alas! have not our dingy brethren been in the habit of eating such rice, generation after generation, from time immemorial? and why should those of the present day find their bane in that which nourished their forefathers? But even granting the possibility of the degeneration of the plant to which so many thousands look for almost their sole subsistence, their suffering by its use will not explain the similar suffering of others who had not swallowed this "deadly poison." Now it is well known that many who have not used rice in any form, either old or new, have fallen victims to the cholera morbus. We recollect two cases which occurred in the persons of individuals who had enjoyed the most perfect health for months previously to their attack, and who had not eaten rice in any form. Indeed, many European and American sailors, shortly after their arrival in India, fell victims to this disease; and it is very probable that few, if any, of these men had eaten rice when they had a plentiful supply of green and esculent vegetables, which are peculiarly delicious to those who had been long deprived of them at sea. But a single instance of cholera morbus, in which the patient had not swallowed the "mortal poison" of Ouze rice, is sufficient to disprove the doctrine of Dr. Tytler; and we have seen many, and have good authority for many more. At the same time, we do not deny that the immoderate use of rice may induce a disorder of the digestive system, especially when we consider that many of the Hindoos eat but one meal a day, and this is often so large as to astonish one who is unacquainted with their habits. Perhaps this practice, taken in connexion with

the debilitating heat of the climate, has given rise to many cases of cholera, and other diseases, in which the digestive system is particularly implicated; but it is insufficient alone to explain the existence of the present epidemic, which finds its victims among men of every variety of habit and constitution, although, as it is with other diseases, there are certain kinds of constitutions, and certain habits, which favour more than others the attack of this malady. In this particular, we differ with M. de Jonnes, who appears to consider all persons equally liable to the cholera of India. We did not find this to be the case in 1818; on the contrary, while vast numbers of the lower classes of natives were daily falling victims to it, those of the higher classes, who used a more nourishing and various diet, were comparatively little afflicted; and its appearance among Europeans and Americans, especially those lately arrived, was a rare occurrence; it was also more difficult of cure in the former instances than in the latter.

These facts throw some light on the nature and cause of the disease. The lower classes of the Bengalese, especially in the cities, are confined to a poor diet, exclusively vegetable, and if they enjoy any variety in their meals, it is only a different method of cooking the same article. We have heard it remarked, indeed, by both Brahmins and Europeans, that in the city of Calcutta, and probably in other places, there were many thousands of natives who had eaten nothing but rice throughout their lives, since they had been weaned from the breast. They are also thinly clad, and exposed to all the vicissitudes of the weather, for their huts, being principally built of bamboos and palm leaves, afford but an imperfect shelter from the rains, which, at certain seasons, are very heavy; and many have not even this poor assylum. We have more than once been obstructed in our rambles through the streets of Calcutta, at night, by numbers of these sleeping wretches, who, like Campbell's exile, had "no refuge from famine and danger."

We all know that proper food, and a certain degree of re-

gularity in rest and exercise, are necessary to healthy digestion, and we know, also, that if, from any extraneous cause, this process is frequently interrupted, the digestive organs become debilitated, and otherwise suffer derangement of their functions, so that a sudden removal of the remote cause of their disorder will not immediately obviate the injury sustained. Hence we might reasonably expect to find, among the poor vagabonds of Calcutta, many instances of diseases, which have their origin in derangement of the digestive system. The cholera, which has been so fatal among these people, is, in every respect, purely a disease of the childpoetic viscera, of which the stomach appears to be the first part affected. The disease is first detected by violent emesis, with pain in the head, soon succeeded by stupor. The evacuations *per anum* are generally, if not always, preceded by these symptoms of gastric derangement. The pulse is very much depressed at the outset of the disease, and continues to become weaker, and less frequent, until life is extinct, unless the patient should be so fortunate as to have obtained proper means of relief. The matter first ejected from the stomach consists chiefly of unchanged food—subsequently it has the appearance of turbid water, and, in some instances, it contains bile. This is rarely the case. The appearance of bile is generally a favourable symptom. The stools are watery, and, after a time, become bloody. The stomach from the commencement of the vomiting, is remarkably insensible to the action of stimuli, and, if the case be a violent one, in the course of a few hours from its commencement the most powerful stimuli, given *ad libitum*, will scarcely produce any sensible effect. We saw one case in which French brandy, warmed and spiced with cloves and red pepper; spt. terebinth: aq. ammon. and tr. opii, were successively employed, and in excessively large quantities, without producing any impression upon the pulse, or interrupting the regular course of the disease. In this case, the patient, as well as we could ascertain, had been ill but six hours, at the time that the first dose was given him. He died in less than fourteen

hours. In another instance, however, in which nearly the same remedies were exhibited, from the second hour of the attack, they were completely successful.

Both of these cases occurred in the persons of American seamen. We have good reason to believe that the first could not be attributed to the use of rice, but we are positively certain that the latter owed its existence to some other cause. The patient, in this case, was a very intemperate man, and had been frequently intoxicated by various kinds of liquors, during the two or three weeks immediately previous to his attack.

We are of opinion, upon the whole, that the disease in question is simply a derangement of the digestive functions, depending, most probably, on debility, especially of the stomach; and, therefore, without requiring the operation of any specific poison, such as Dr. Tytler's "Ouse rice," it may be induced by any causes which either directly, or indirectly, debilitate the system, so as to impair the powers of the digestive apparatus; and we know that, in tropical climates, causes, equal to these effects, abound.

We must also differ from M. de Jonnes on another point, namely, that the Indian cholera is equally prevalent at all seasons of the year. According to our own observations, and those of the medical gentlemen resident in India, with whom we have conversed, the state of the weather had a very marked influence on this disease, which was sometimes perceptible from one week to another. During the hot season many deaths occurred from cholera morbus, and but few after the S. E. monsoon set in, and spread over the land the cool refreshing sea air.

With regard to the means of curing this disease, a few words only are necessary. It is evident that a disease capable of running through its fatal career so rapidly as the Indian cholera does, can be successfully opposed only by a prompt exhibition of the most active remedies. The practitioner who attempts the treatment of such a disease by a cautious

essay of the powers of his remedies, and the capability of the constitution of his patient to bear them, will generally find that he has withheld, through timidity, the remedy which, delayed for a short time, cannot be effectually applied by the boldest hand. It is impossible then to effect any thing by a circuitous method of practice, such as may be admissible in other diseases, whose tardy course render them capable of successive attacks at each of their vulnerable points, ere they can arrive at the consummation of their evil tendency.

The stomach appears to be the only medium through which we can make an impression on the disease in question. It appears, indeed, to be, as we have heretofore said, the organ chiefly interested. Its natural action has been suspended, and it can only be recalled to its sensibility, and have its healthy functions restored, by the application of some remedy sufficiently powerful to overcome the morbid impression which has been made upon it. All those substances, therefore, which are capable of stimulating this viscus to the prompt discharge of its functions, when in a healthy state, may be useful in its present debilitated condition. Many of the middle simple stimuli would, in all probability, be sufficient to check the progress of the cholera in its incipient stage; but so rapidly do the vital powers succomb to its influence, that, after it is completely established, it is tangible only by the heavy artillery of medicine.

There appears, also, to be a morbid irritability of the intestines, so that the presence of matter, the most inoffensive to them when in health, now urges them to the most inordinate efforts to expel their contents, while they are negligent of all their other functions. Hence, opium is found to be very useful in allaying the nervous irritation, and thus putting a stop to the excessive evacuation. This is best given in cholera, as in all cases where it is desirable that it should act promptly, in the form of tincture; and as the stomach has lost much of its sensibility, as we before observed, it must be given in much larger doses than could safely be exhibited under ordi-

nary circumstances. Other stimuli, such as ardent spirits and spices, have also been found to be very useful auxiliaries to the opium; and, in some instances, much benefit has resulted from the application of sinapisms over the abdomen. The patient should be covered by warm blankets, and have the surface of his body frequently rubbed with a flesh-brush: in a word, every means should be resorted to for the purpose of exciting a determination to the surface.

The great emaciation consequent to a few hours continuation of cholera, indicates the existence of a preternatural activity of the absorbents. We cannot, indeed, account for the vast quantity of fluid that is discharged from the stomach and intestines, without supposing that it is supplied by these vessels. After the violent emesis and catharsis have been completely checked by the above mentioned means, a moderate dose of castor oil, combined with a few grains of calomel, should be given, to prevent the torpid state of the bowels consequent to the opium which had been exhibited. Subsequently, the treatment should consist of nutritious food, with a moderate use of port wine, or brandy, and rest.

The observation of M. de Jonnes, that all constitutions were equally liable to the attack of the cholera of India, and that no system of living would diminish the liability to it, reminds us of an observation of Dr. Armstrong, in his treatise on Typhus, in which he alludes to the habits of living most likely to exempt strangers in India from the bilious disorders so prevalent and so fatal in that region. Although somewhat foreign to the subject of this article, we presume that a few remarks on this very important question will not be deemed impertinent. They are not directed against the opinions of any certain individuals, but against a very common prejudice, and one which has unaccountably found its way into many valuable treatises on diseases of warm climates.

In the advice which is given for the preservation of health, in hot and sickly climates, attention to diet is generally recommended, and very properly; but this vague language is

commonly interpreted as an interdiction of all manner of rich, stimulating food, condiments, &c. In some instances, an exclusively vegetable regimen is recommended; and the stomach is refused any assistance, save that of water or thin beer, to its digestion of this ungracious aliment.

Now, it is perfectly consistent with reason, and with our knowledge of the influence of the digestive organs upon the system in general, to suppose that a predisposition to bilious fevers may be the result of improper habits in regard to the kind and quantity of food. That kind, for example, which is difficult of digestion, and yields but a small proportion of nutriment, may impair the strength, and derange the healthy functions of the system, by debilitating the stomach and other viscera employed in digestion. Secondly, the digestive power of the stomach may be so weak as not to be able to overcome the common chemical laws, to which all matter is subject, and correct the tendency of the food to fermentation and putrefaction. And that these processes do sometimes take place in the stomach, is, we believe, not doubted.

In warm climates, when the system is relaxed and debilitated by excessive perspiration, and other causes, the powers of the digestive organs are diminished. If, then, healthy digestion be desirable, by what rule do we present to the stomach that kind of food which requires the greatest time, and consequently the greater exertion of the chylopoitic vicera for its assimilation to the body?

As far as our observation has extended in the torrid and temperate zones, we have not seen that the plan of vegetable diet, and abstinence from stimulating drinks, is at all calculated to preserve the system from the inroads of disease. On the contrary, we have seen so many instances of its failure, that we are inclined to consider it as positively pernicious. We have had frequent and ample opportunity of comparing the effects of this system with the opposite one of generous diet, and the use of condiments and stimulating drinks, and the result has been, without exception, favourable to the latter.

We are aware, that in recommending the use of spirituous drinks, we tempt the wrath of a certain class who habitually anathematise all such *indulgences.* We are well acquainted with all the imposing arguments which have been advanced in condemnation of every practice which may lead to intemperance; but we would respectfully remind our cautious opponents, that in recommending the *use* of ardent spirits, we are not to be understood as favouring its *abuse.* It is a medicine, when properly used, and, like all other medicines, it becomes a poison only when misapplied. But to pursue our argument—we have stated, that all the powers of the system, vital as well as physical, become enervated in warm climates. Does any one doubt this—let him observe the effects produced upon a strong and healthy man, from a northern clime; by a few years residence within the tropics, where the ruddy complexion of health declines into the melancholy hue of disease, where activity is lost in slothfulness, and strength gives way to imbecility. We may take a wider view of this subject. We find, universally, that the inhabitants of warm countries are inferior to those of colder climates, not only in physical power, but also in mental energy, and in the duration of their existence.

It was, no doubt, intended that all parts of the earth should be peopled—go where we may, we find animals which are supplied with food adapted to their peculiar necessities.

In cold climates there are fewer vegetables than in warmer regions, and we find among those of the latter a much greater proportion of spicy, stimulating plants. Why are the warmer climates thus supplied with productions of this kind? Nature does nothing in vain—she does not place an animal in one part of the world who must depend upon the productions of another for his subsistence; and with respect to omnivorous man, he finds in every land those articles of diet, which the peculiarities of the climate render necessary to the preservation of his health. In cold climates, where his strength is not exhausted by perpetual perspiration, he does not require those stimulat-

ing condiments which are necessary to assist his languid digestion in tropical regions.

Whatsoever effect these considerations may have upon our opinions respecting the dietetic system which a visitor to a warm climate ought to adopt, we do not depend solely upon them; for however plausible they may be, we have the better, the unanswerable testimony of experience to the same effect. During many summers, spent in hot and sickly countries, we have devoted much attention to this subject, the result of which is, that generous food, and a proper use of stimuli, proportioned in quantity to the exercise and to the heat of the weather, affords much better protection against violent disease than the cautious system pursued by some, who destroy all their energies by starvation and want of exercise; fearing that a latent fever is reposing in the system, and may be roused into dreadful activity by a drop of brandy, or an honest meal.

We have also observed, in those who live well, that the bowels are more regular than those of the other class; and this we know is an important criterion of the state of the system, as regards its tendency to febrile disease.

The early occurrence of vomiting, in cases of bilious fevers, goes far to prove that the stomach is affected previously to the existence of any apparent symptom of disorder. It has most probably been debilitated so as to allow of the formation of acid matter from the food which had not been properly digested. In warm countries, many complain of an uneasy sensation in the stomach immediately after rising in the morning, which is removed by eating. It is customary with such to use bitter tinctures and other forms of stimulus in the morning, from which they derive immediate relief. Although this practice has been warmly reprobated by those who have never experienced its good effects, and whose prejudices prevent them from seeing its propriety, yet we are inclined to think that, under certain circumstances, it is extremely beneficial.

In the year 1818, when great mortality prevailed among

the crews of the European and American ships lying at Calcutta, we made an experiment with the crew of the ship Bainbridge, the effect of which was so satisfactory, that we are induced to mention it here, in support of the position we have taken.

A quantity of tincture of gentian was made with good French brandy, of which each man of the crew was required to take a small wine-glassful every morning before breakfast, and another a short time before dinner. The consequence was, that after this practice was established, not one instance of serious disease occurred on board this ship, while, among the other vessels, not one could be found that had not lost several of its crew. It is possible that other circumstances concurred to the remarkable exemption of the crew of the Bainbridge from disease, but we have sought them in vain; for they lived in all respects in the same manner, and were subject to the same exposure as their less fortunate neighbours. We have, indeed, observed in other countries, during a sickly season, in hot weather, a similar practice pursued, and apparently with good effect.

Dr. Johnson, in his essay on tropical hygiene, strongly reprobates the *abuse* of stimulating drinks, in which we cordially concur with him; but, unfortunately, he has neglected to treat of their *use*. Dr. Johnson brings, in support of his doctrine, the case of a very good-humoured, lively gentleman, at Prince of Wales' island, who enjoyed excellent health without the assistance of stimulating drinks. We have not the least doubt that such instances exist; but, on the other hand, we could bring numerous instances in which the health has remained unimpaired for many years, under the other extreme, with regard to drinking; in some of which the individuals never passed a day without a rich and hearty dinner, and seldom went to bed sober.

We have seen many instances in which the stomach has become debilitated, by the use of pure water, so as to reject that fluid, when it has been freely used to abate the thirst

consequent to profuse perspiration. Sometimes it would return from the stomach, as insipid as when it was swallowed, which seems to prove that its rejection was not the effect of any acrid matter in the stomach. We have frequently experienced this effect of cold water, and we have never failed in obviating it, by a small quantity of some stimulating drink, such as a glass of wine, or brandy and water. In recommending also a vegetable regimen in preference to animal food, to residents in India, Dr. Johnson very innocently overturns all his theory by contrasting the fat hearty Brahmin, who indulges in the pleasures of the table, with the lank Cooly, who is confined by necessity to the simplest food. The difference between the higher and lower casts, in external appearance, is, indeed, too remarkable to escape the most careless observer; while the latter are generally small and meager, and of very dark complexion, the former afford many examples of robust and portly men, and we have seen among them some of the finest models of the human figure.

Upon the whole, nothing can be more unphilosophical than an attempt to prescribe any system of regimen which will be applicable to all, unless we first reduce all the various constitutions of men to one scale, and confine all men to one given proportion of exercise and rest. In excluding either animal or vegetable matter, we would act in an injudicious extreme, and in the use of drinks, we may fall into the same error, by confining ourselves to pure water, on the one hand, and by a free indulgence in inebriating potations on the other.

T. M. H.

Art. II.—*History and method of cure of the various forms of Epilepsy.* By John Cooke, M. D. F. R. S. &c. &c. 8vo. p. 235. London, 1823.

The present work affords us no opportunity of amusing our readers with ingenious speculation, but it enables us to serve their interests much better by presenting them with an analy-

sis of an excellent practical work, on a disease that occurs in every clime, and among all classes of the human family.

Although Dr. Cooke has added but little from his own experience to our knowledge of the nature and causes of epilepsy, the judgment and industry which he has displayed in embodying within a small space, the observations of both ancient and modern authors, especially with regard to the remedies for this disease, entitle him to a rank among the useful friends of medical science.

In the history of epilepsy, the author presents us, in a condensed form, all the causes to which this disease has been ascribed; and with a number of remarkable and authentic cases, illustrative of the variety of sources in which epileptic affections have their origin. These are so dissimilar as to leave no foundation for the belief that the disease can be ideopathic, but they seem rather to prove that it is always a symptomatic affection, and that its primary cause may be located in the brain, the stomach, the intestines, and sometimes in a remote and unimportant nerve. In the following passage, however, our author advocates the opinion that a predisposition to epilepsy may exist in certain constitutions.

"With regard to predisposition to epilepsy, we have no clear and distinct knowledge. We cannot ascertain its nature, nor even its existence, in many cases, or whether or not it be necessary to the formation of the complaint. Some pathologists are of opinion, that it is not necessary, and that epilepsy may be produced, and in some cases has been produced, by a new and strong impression in a perfectly well organized brain and nervous system, and independently of predisposition. Be this as it may, I am inclined to believe, nay, I think it evident, that a certain constitution of brain and nervous system exists in particular persons, which renders them especially liable to this disorder; that there is a much greater sensibility, irritability, or what Dr. Cullen calls mobility, of brain and nerves, in some persons than in others, which constitutes predisposition to epilepsy.

"We see some persons easily elated by hope, and depressed by fear, and passing quickly from the one state to the other; easily pleased, and prone to gaiety, and as easily provoked to anger, and rendered peevish, and liable to strong emotions from slight impressions. This is the temperament *qui colligit et ponet iram temere, et mutatue in horas;* this is the *varium et mutabile fœmina.*

Whether, in all cases of idiopathic epilepsy, a particular predisposing constitution exists or not, it is certain that the disease is excited in some persons, by causes which do not produce it in others. A certain confirmation of cranium, in particular, seems to predispose to epilepsy. Leduc remarks, that the head of epileptics is larger and the skull thicker than in healthy persons; and Lorry and others confirm this observation.

The circumstance of epilepsy occuring in successive generations of the same family, is also strongly in favour of the doctrine of permanent predisposition.

Dr. Cooke observes that in whatsoever the predisposition to epilepsy may consist, it is evidently increased by a plethoric state of body. In this he agrees with Hypocrates and Van Swieten, as well as with many modern authors. "In almost every case," says our author, "which I have had an opportunity of seeing, the disease has occurred in full habits and sanguine temperaments." He adds, however, Dr. Prichards' opinion, that no particular temperament is peculiarly liable to epilepsy, and that this gentleman "has witnessed its appearance in every variety of habit, from the most exquisite examples of the sanguine, to the most marked melancholic."*

"The *exciting* or *occasional* causes of epilepsy," says Dr. Cooke, "are of various kinds, and act in various ways. Some are stimulants, producing, it is supposed, an increased action or energy of the brain; and others sedative, operating so as to diminish that action or energy. Of these causes, some act directly upon the brain, others produce their effects primarily upon distant parts, and secondarily upon the brain.

"Among those which act primarily upon the brain, the chief are mechanical causes, such as malconformation and injuries of the cranium, tumours, depressed bone, sharp pointed spiculæ, situated on the internal surface of the skull, hydatids, congestions distending the vessels of the brain, changes in its structure, or diseases in its substance, and effusions of various kinds," &c.

We cannot agree with our author in arranging these *mechanical causes* of epilepsy, under the title of "*exciting* or

* Prichard or Nervous Diseases.

occasional" causes, under the supposition that they merely determine a latent predisposition to activity. We think that they are amply sufficient to produce epileptic symptoms without the aid of any other circumstance; and if this be granted, it is not strictly philosophical to seek a more remote cause, such as a predisposition, with which they have no natural connexion.

Partial plethora is mentioned as the cause of most frequent occurrence, "after epilepsy, the vessels of the brain are often found overloaded with blood, and in a state of great distention. The celebrated anatomist Meickel, declares, that in his numerous examinations of the brain, he had never seen such an *engorgement* as that of an *epileptic* in the hospital at Berlin." But how does this harmonise with what follows. "There are other causes which also produce their effect primarily upon the brain, but probably by a *sedative* power, or by diminishing the energy of that organ, or, as Dr. Cullen calls it, by collapse. These are, depressing passions, fear, terror, grief, disagreeable impressions on the imagination and senses; the recollection of former paroxysms,* or their causes; or whatever greatly *weakens the action of the heart*, as hemorrhage, or other excessive evacuations."

Under these circumstances we could hardly expect to meet with cerebral engorgement: but it is one of the characteristics of this singular disease, that the most opposite causes seem equally capable of exciting it; and neither the plethoric *bon vivant*, nor the abstemious *decharné* are exempt from its attack.

Among other causes to which our author traces epileptic affections, are the sudden suppression of long established discharges from the body, and of cutaneous eruptions, worms,

* We have witnessed an instance of the efficacy of this exciting cause. The patient was a young gentleman of delicate constitution, and had been subject to epilepsy for a considerable time. Frequently, the slightest hint respecting his disease, or a visit to the spot where he had recently suffered a paroxysm, would throw him in convulsions. H.

and other offensive matter in the intestines, injury done to the nerves, or a morbid state of them.

"In the Edinburgh Medical Essays and Observations, a case is related of a violent epilepsy which frequently occurred, which was produced by a hard cartilaginous substance, of the size of a large pea, situated upon a nerve. That this was really the cause was evident, as the disease ceased on the extirpation of the tumour. In the same journal we have an account of epilepsy depending upon a calculus of an irregular figure, about the size of a nut, (*quere, a hazel-nut or a cocoa-nut*) pressing on a branch of the sciatic nerve; and another, in which the *par vagum* was compressed by a concretion of a similar kind."

He also refers to the observations of M. de Maisonneuve on gastric epilepsy, for the example of eighteen men, who were shipwrecked, and of whom fourteen fell victims to epilepsy, occasioned by the use of sea water and the indigestible food to which they were reduced, in evidence of this disease being sometimes the result of irritation of the nerves of the abdominal viscera.

Dr. Prout is also quoted, and his opinion, that acrid solids or fluids, bile, but principally worms in the stomach and intestines, are the direct causes of that nervous irritation which gives rise to epilepsy; and this opinion is supported by the appearances exhibited by these organs when dissected.

"On opening the bodies of epileptic persons, particularly of those cut off by the disease in the fit, this writer found the arterial system strongly developed in the small intestines; while the larger intestines were seen containing a greater or less number of worms, particularly of ascarides. These worms were chiefly found in the cæcum and the ascending portion of the colon. The mucous membranes of these bowels were more or less inflamed, according to the vigour of the patient and the duration of the disease."

Van Swieten is also quoted for a case in which a boy died of epilepsy, apparently occasioned by a worm perforating the duodenum: and Wepffer for an instance of the disease excited by a tape-worm, in which a cure succeeded the discharge of the animal: and another case in which the patient, after suffering for several years, "with such frequent paroxysms as to

occasion a total imbecility and absolute loss of memory," recovered her health and her faculties on voiding a worm.

M. Fabricius of Helmstadt, Bartholimus and La Motte, are referred to as authority for some instances of epilepsy arising from the irritation occasioned by biliary concretions in the gall bladder, and calculi in the kidneys and ureters. Lastly, Dr. Cooke mentions poisons and irritating articles of food taken into the stomach, as causes of epilepsy, of which some instances are related.

The uterus is next mentioned as the seat of the primary cause of epilepsy. "Some writers," says Dr. Cooke, "mention epilepsy as occasionally produced by pregnancy. Tissot mentions several cases of this kind which fell under his own observation. In one of them, the patient had, in three pregnancies, experienced an epileptic paroxysm almost every week, till the motions of the child were perceived."

During the pregnant state, it is true, that women are subject to a variety of complaints, which seldom or never appear under other circumstances. If we can rely on the state of the mind as indicative of nervous derangement, or disposition to nervous diseases, it is not improbable from what we observe in some women, that pregnancy may induce that form of nervous disease under consideration. But simple pressure on a nerve has been mentioned as a cause of epilepsy, of which some instances have been given above. If this be the case, may we not refer those instances of the disease which have occurred during pregnancy, and which have been referred to some mysterious influence of the gravid uterus upon the nervous system, simply to compression of some branches of the lumbar or sacral nerves by the head of the fœtus? In support of the affirmative of this question, it may be remarked, that the disease disappeared after parturition, and indeed upon a slight motion of the child, which, in all probability, relieved the suffering nerve from compression. With a case of this kind before us, it is impossible to arrive at a demonstration of the actual cause of epilepsy during pregnancy. We must,

therefore, be content with probability. With regard to the compression of a nerve by the fœtus, we know that this may take place, and we have good reason to believe that it frequently does; and in the cases of epilepsy, where the disease immediately disappeared on the removal of tumours in the course of the nerves, we have a ready solution of the phenomenon of its disappearance during pregnancy after the motions of the child. But if we maintain the idea that the disease is connected with the state of the uterus, we can see no reason why it should be in any degree affected by the motions of the fœtus, inasmuch as such motions can have no effect on the state of the uterus. We have, then, one cause whose efficiency is established, and which frequently exists during pregnancy—why should we grope in the darkness of mystery for another to the same effect?

"With respect to the connexion between nervous disorders," says Dr. Prichard, "epilepsy is a distinct disease from apoplexy and palsy, and yet its relation to both is very near; and they all frequently pass into each other. Persons who have partially recovered from a recent apoplexy, are often assailed by convulsions, which display most of the phenomena of epilepsy; and fits of the genuine epileptic character frequently occur after an attack of hemiplegia. On the contrary, persons who fall victims to repeated fits of epilepsy, perish under all the symptoms of apoplexy; and others who recover from a severe fit, or from frequently repeated fits of epilepsy, are often found to labour under hemiplegia, or other modifications of palsy. Sometimes persons who have long suffered under epilepsy, lose this disease and become permanently paralytic."

Dr. Prichard also thinks "that insanity is still more intimately connected with epilepsy. In very severe and inveterate cases of epilepsy, the paroxysms of this disease are often followed by attacks of maniacal delirium, which are generally of the most violent kind. These fits of madness most commonly abate in a few days after the epileptic attacks have ceased; in other instances, however, the maniacal state is of longer continuance, and epilepsy is often the harbinger of a permanent and hopeless insanity."

Thus we find that epilepsy has so many connexions with other diseases, that we can scarcely admit it in any instance to be ideopathic. It appears to be purely a result of nervous irritation arising from any cause, even mechanical injury. It

has also been observed to attend dentition and exanthematous disorders in children, and irregular menstruation in females.

Dr. Cooke proceeds to investigate the proximate cause of epilepsy; and after alluding to the ancient opinion of a pituitous humour in the ventricles of the brain, and the symptoms of the disease produced by an effort of nature to relieve the brain from the pressure of this fluid, he mentions the opinion of Boerhaave and Van Swieten, that the disease resulted from "too great action of the brain on the nerves of motion, without any on those of sensation," and then introduces more at length Mr. Mansford's theory, which we think possesses some ingenuity, though it is very hypothetical. This gentleman supposes that the proximate cause of epilepsia is an accumulation of the electrical fluid in the brain. He takes it for granted that the electrical and nervous fluids are similar, and that organized matter can control electricity so as to restrain it from its tendency to diffuse itself equally among connected bodies not under the influence of the vital principle. Muscular contraction, therefore, is produced by a flow of the electrical fluid from the brain, which is its reservoir, along the nerves to the muscles, by an effort of the will; but in epilepsy, the will is unable to control the distribution and action of this fluid. "The vital principle being absolutely overwhelmed and losing its command, the motive powers of the system become for a time obedient to those laws which would govern them in any other situation.—The vital principle being freed from the load which threatened its existence resumes its seat and power."

It is impossible to reason with any decisive effect on an hypothesis like this—it is too much like seating the soul in the pineal gland.

The opinion to which our author seems to lean, for he does not positively express his bias, is that of "derangement of the organization of some part or parts of the brain and nerves within the cranium."

"In epilepsy," says Dr. Cooke, "sensation is impeded, and motion is morbidly increased, and is no longer performed in obedience to the will.

Now it is not unreasonable to suppose that this may depend upon a deranged state of the organization of certain parts within the cranium, because we can have no doubt that whatever be the nature of the sentient, willing, and first moving power, it acts and is acted upon through the medium of organized matter: and, because on dissection after epilepsy, derangement of the organization of parts within the cranium have been very often actually found. In order that the perceiving power may act perfectly, it would seem that its instruments must act perfectly. For the perfection of vision, the organ of sight must be unimpaired. If the eye be obscured, sight will become obscure; if its transparency be restored, distinct vision will be restored. Similar observations might be made respecting the other senses."

To the opinion of those who place the proximate cause of epilepsy in diseased structure of the parts within the cranium, it has been objected, that in many cases, on examination after death, of persons who have been affected with the complaint, no marks of disorganization whatever, after the most minute and careful investigation, could be found. It does not, however, follow, that no such derangement of organization exists. The structure of the various parts within the cranium is so exquisite, and the particular uses, or functions of them so entirely unknown to us, in health, that derangements of the greatest importance may exist in them without the possibility of our being able to discover them.

To the opinion that epilepsy immediately depends upon derangements of the structure of the brain, as abovementioned, has also been objected that much of that organ may be destroyed without producing any symptoms of the disease: but this does not appear to me to be an insuperable objection, for I believe that the facts on which it depends, relate only to the destruction of the parts of the *cerebrum*, and it seems highly probable that the organized parts immediately connected with sensation and motion, are placed in the lower region of the cranium. This opinion is confirmed by some experiments made by Sauvage, and by the observations Wenzel already particularly detailed. Sauvage observes, that if the denuded brain of an animal be perforated with a sharp instrument, no sensation will be occasioned; but if the in-

strument reach the origin of the nerves, or medulla oblongata, an epileptic paroxysm will be produced. Now, the parts pointed out by Mr. Wenzel as the principal seats of organic derangement in epilepsy, and those by the injury of which Sauvage was able to induce the disease, very well correspond. The experiments of the last mentioned author seem to confirm the extraordinary observations of the first.

If, however, we could prove that in very many, or even in all cases of epilepsy, these derangements of organization had taken place, it is extremely difficult for us to understand how such derangements could produce some of the symptoms of the disease. That sensation and motion should thus be abolished, we may conceive; but how are we thus to explain the increase of motion which takes place in epilepsy? The theory abovementioned by Boerhave and Van Swieten, or the notion of a spasmodic increased action of the parts necessary to motion, would perhaps afford a better *rationale* of this phenomenon. Notwithstanding the pains which many ingenious physiologists have taken, in endeavouring to understand this subject, it still remains very obscure; and perhaps we must in candour admit the truth of what has been said by the learned author of a dissertation on epilepsy, who, speaking of its proximate cause, observes, "*Est quasi terra incognita, in qua quisque pro voluntate sua vagatur et viam diligit jam factam, aut facit. Auctores de hac re multas plausibiles et populares fabulas effinxerunt; hæc vero omnia novimus esse nihil.*"

The author's short chapter on the *Diagnosis and Prognosis* of epilepsia, presents nothing new or remarkable beyond what is familiar, we presume, to every practitioner. The methods of treatment, however, occupy about two-thirds of the volume, and on this subject Dr. Cooke has collected every thing of importance, from both ancient and modern sources.

When those symptoms which are generally observed to be the precursors of an epileptic fit occur, or when the fit is

completely established, in what the author calls ideopathic epilepsy, he recommends early and active depletion by blood-letting and purging, which may be carried to great length in patients who are favoured by youth and robust habit. In aged patients he thinks that cupping is more advisable than general blood-letting. This, however, is merely in obedience to the general rule, that a young and strong animal can lose, without injury, more blood in a given time than an old and debilitated one. We believe that in some instances of epilepsy occuring in aged patients, it would scarcely be prudent to depend upon so gentle and inefficient a remedy as cupping. If there be an engorgement of the vessels of the brain, this method of abstracting blood is too gradual to produce much effect upon the general circulation. As an adjunct to the lancet it may in some instances be beneficial. The purgative, he very properly remarks, should be of the most active kind, and if necessary, its operation should be promoted by stimulating enemata.

Aretæus recommends an emetic, previously to the accession of the fit, and Frazer says that "the patient, if an adult, should swallow from thirty to forty drops of laudanum, in a draught of camphorated emulsion," immediately on the appearance of the premonitory symptoms. "The common effect of this is found to be the complete prevention of the paroxysm, and the restoration of the patient to his usual health. When the medicine fails in entirely preventing the accession of the fit, its violence and duration are uniformly mitigated by it."

With regard to the practice of Aretæus, Dr. Cooke does not approve of the use of emetics in this disorder, especially in plethoric habits. He confesses his want of experience in the use of opium.

We think that our author has passed over this plan of treatment with less ceremony than it deserves. The proper cure for epilepsy must depend upon the nature of those causes which produce the disease. For example, if it arises from

worms, or any form of irritating matter in the intestines, cathartic evacuants are peculiarly indicated: if from plethora, there is perhaps no class of evacuants which would not be in some degree useful. But if the disease arises from nervous irritation by a local cause, as a wounded or compressed nerve, we might reasonably expect to derive great advantage from a variety of stimulating articles, and particularly from opium. In fact, opium, among other narcotics and antispasmodics, is mentioned farther on, (p. 169,) as having been strongly recommended in the cure of epilepsy.

In those cases of epilepsy in which the fit is preceded by the *aura epileptica*, the author quotes Aretæus, Galen, and Cullen, in recommendation of the ligature between the point where the *aura* is first felt, and the brain. This method, it is stated, has frequently prevented a paroxysm of the disease. Dr. Cooke, in his directions for the treatment of a patient, during a fit of epilepsy, after giving the usual precautions for restraining the muscular exertions, guarding the tongue from laceration, &c., seems inclined to dissuade us from the use of the lancet, upon the authority of Heberden, who says, that "bleeding during a paroxysm, is calculated to lessen the strength of the patient, but not the power of the disease." He also introduces an observation of Tissot, that spontaneous bleeding from the nose has occurred during the fit, without mitigating its violence, and he also alludes to the difficulty of performing the operation upon a convulsed patient, as an argument against this practice.

We regret that we are at variance with our author on this point; with regard to the remark of Heberden, it has a specious appearance, but we do not think that it will withstand the test of examination. It is too commonly the case, especially among the ignorant, to suppose that a portion of blood taken from the veins of an animal, is so much taken from his strength, and that the loss in the latter article is permanent: nothing can be more absurd. In the first place, if the strength was in direct proportion to the quantity of blood,

still, as the blood abstracted is soon reproduced, no important injury could be sustained, unless the depletion be carried to great excess; secondly, when the whole muscular system, as well as the arterial, is agitated by excessive action, there is much more danger of succeeding debility by a continuance of the convulsion, than could reasonably be expected from the loss of as much blood as would arrest the fit, even though syncopy should be repeatedly produced. If we imagine that the brain is oppressed with plethora, the propriety, nay, the necessity, of blood-letting is beyond all question. As to the difficulties which attend the operation upon a convulsed person, they are easily overcome by a skilful hand.

A case illustrative not only of the good effects of blood-letting, but also of the exhibition of opium and camphor, is quoted from Mr. Earle, by our author.

"A short time since I witnessed a singular result from the local abstraction of blood, in a case of long standing. A lady, between thirty and forty, had been subject to fits from her infancy, for which she had been treated with strong stimulating nervous medicines, and very full diet. She generally experienced a succession of fits, to the extent of six or eight; and the attacks were always preceded and followed by severe head-aches, and much convulsive twitching. I found her just coming out of a second fit, and quite insensible; her countenance was crimson, and her eyes quite feretty. The determination to the head was so very marked, that I immediately recommended cupping from behind the ear, which was performed to the extent of sixteen ounces. She speedily became sensible, and had no return of fits; but instead of falling into a profound sleep, which was the usual consequence of an epileptic paroxysm, she became extremely talkative, and her imagination was so unusually active, that she soon began to speak as if conversing with persons who were not present. By reasoning with her she was made sensible that she was talking incoherently, but soon relapsed again. I was called to visit her in this state, and by giving her a powerful dose of camphor and opium, she very soon became composed, and fell into a quiet sleep, from which she awoke perfectly well and free from head-ache. The effect of the sudden abstraction of blood from a surcharged brain, in this case, was very curious, and might lead to much speculation; but as facts are rather desired than theories, I give you the simple narrative."

With regard to Tissot's remark of the failure of epistaxis to remove a fit, we do not think it by any means conclusive

nor, indeed, worthy of any weight. It seldom happens that a quantity of blood escapes in this way, sufficiently large to make a sensible impression upon an epileptic paroxysm. Besides which, as we know that the effect of blood-letting depends upon the rapidity of its abstraction, as well as the quantity drawn, we could expect but little effect from epistaxis. The best writers on this disease recommend bleeding in the paroxysm, and we believe that the practice is sanctioned by universal adoption. We can safely say that we have, in various instances, witnessed its good effects in diminishing the violence and the duration of a paroxysm, without the interference of any auxiliary.

On the subject of the radical cure of epilepsy, we feel happy in coinciding with our author:

"For the radical cure of epilepsy, we trust to means to be employed in its intervals. Before I enter on a consideration of these means, I wish to observe that, in this disorder, it is of consequence that the epileptic habit, if it may be so called, should be broken, by avoiding every thing associated with the complaint, or that might bring the thoughts of it to the mind of the patient. As the traces of ideas which are not from time to time renewed, says a French writer, gradually become entirely effaced, so the epileptic aptitude may be destroyed.*

"The predisposition to epilepsy is sometimes hereditary; in which case it is particularly difficult to cure. Indeed, some authors consider the disease, under these circumstances, to be incurable. Boerhaave and Van Swieten speak very confidently on this subject. The former says positively, that hereditary epilepsy is never cured; and the latter observes, that it has been reckoned incurable by all physicians, and that, perhaps, it is not more in our power to remove this latent impression, communicated from parents to their offspring, than to prevent the teeth or the beard from growing. This doctrine, I believe, is not true; and, indeed, Van Swieten admits that circumstances may prevent the occasional causes from rousing the latent predisposition. At any rate, *it is mischievous because it discourages the exertions of the physician.*

"The treatment which the most experienced physicians have recommended, with a view to diminish or remove plethora, general or partial, are

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* Tissot. See, also, Cullen.—Ed.

depletion by blood-letting and purging, emetics, blisters, setons and issues; the restoration of discharges which may have ceased, or been artificially stopped, and by proper diet and exercise. The most powerful and the most speedy depletion, as I have already had occasion to observe, is produced by blood-letting and purging; and in all cases of epilepsy connected with plethora, I believe these to be the most efficient remedies."

Dr. Cooke illustrates the beneficial effects of lessening the determination to the head, by a remarkable case related by Mr. Earle, wherein the paroxysm, which Dr. Cooke remarks, resembled apoplexy, was frequently arrested by compression of the carotid arteries. Mr. E. has, also, in the history of this case, hinted at the propriety of obliterating by ligature, one or both of the carotids, in such cases, (we presume) of epilepsy as resemble apoplexy, as a means of permanent cure,—for in those which result from irritation in parts remote from the brain, or those which are connected with debility, and are not attended by cephalic plethora, no advantage could be expected from this practice.

In those cases of epilepsy, if any such exist, which arise simply from a determination to the head, this practice may be advantageous. But we have already expressed our doubts with respect to the existence of ideopathic epilepsy.

Dr. Cooke subscribes to the doctrine, that no particular remedy is to be depended upon for the complete cure of this disease, although there may be many means of arresting a paroxysm. Dr. Prichard recommends a free use of evacuant remedies in nervous disorders generally,* and Cullen advocates an abstemious diet as necessary to the safety of epileptic patients. Our author, however, speaks of epilepsy arising from debility, in which quite a contrary course is indicated. In such cases, generous diet, fresh air, tonic medicines, exercise, and cold bathing are recommended.

Among the tonics which have been successful in epilepsy, the nitrate of silver holds a high rank in our author's esti-

* See also "Hamilton on Purgatives."—Ed.

mation. It appears that this medicine has generally been given too cautiously, on account of its causticity. Dr. C. relates numerous cases in which it was given to the extent of eight grains daily, without inconvenience to the patient. Many names are quoted by Dr. C. as authority for its efficacy, in that form of epilepsy, which he calls ideopathic. Its property of darkening the skin, however, especially those parts most exposed to light, would, in some instances, be a serious objection to it. It is a curious circumstance that this discoloration of the skin sometimes takes place after the use of the nitrate of silver has been discontinued for weeks, and even months. Dr. Roget mentions a case in which this effect remained without any alteration for twelve years—so that it might fairly be presumed to be permanent. The preparations of copper, zinc, and lead, are also spoken of as remedies for epilepsy.

In epilepsy occasioned by worms, Dr. Cooke mentions turpentine as a remedy, from which the happiest effects have been obtained. It is necessary, however, in the generality of cases, to assist it by cathartics: in fact, these should never be omitted in any form of epilepsy.

"In enteritic epilepsy, attended with torpor of the bowels, Dr. Prichard strongly recommends the use of oleum terebinthinæ, in doses of half a drachm, or one or two drachms with aqua carui, or cinnamomi made into an emulsion, by means of honey, or mucilage. He thinks that this medicine has some specific effect in cases of fits, depending upon enteritic irritation. He is also of opinion that *argentum nitratum*, and those medicines called nervine and antispasmodic, are chiefly useful in epilepsy of this kind. In enteritic disorder, attended with diarrhœa, he recommends absorbents, *hydrargyrus cum creta*, aromatic powder, and infusion of rhubarb, cloves, &c."

The last method of treating epilepsy which remains to be considered, is the application of the actual cautery, or the trephine, to the cranium. One of the most powerful means made use of by the ancients, was the application of heated irons. Aretæus, Galen, and Celsus, particularly recommended, and very boldly employed, this remedy in epilepsy: and

Hippocrates, speaking generally, says, when medicine fails, steel may cure; when steel fails, fire may cure; when fire fails, the disease is incurable. In epilepsy, says Aretæus, the application of fire to the head is necessary, and will be found to be of great use. He directs that the bone should be perforated as far as the diploe, and that afterwards cerates and cataplasms should be used until the membrane is separated from the bones. The naked bones should be trepanned until the black and thick membrane is found. He seems to intimate, that a discharge from the parts should be kept up for some time. Celsus was a strenuous advocate for the actual cautery. After scarifying the back part of the head, and employing cupping-glasses, he directs the application of the hot iron.

In some instances, both the cautery and trephine appear to have been employed by the ancients in attempting the cure of epilepsy; but the moderns, until very lately, did not venture to try the former of these powerful remedies. The operation of trepanning was, however, recommended in some cases by Boerhaave and others of his time. Boerhaave seems to have adopted the ancient opinion, that peccant humours sometimes cause epilepsy, and that the direct evacuation of them may remove it; and Van Swieten observes, that extravasated blood, by compressing the enecphalon, has sometimes produced the disease, which has been cured by the trephine. Tissot recommends the operation of trepanning under certain circumstances of the disorder; and we have an account in the memoirs of the Royal Academy of Surgery, of a case of epilepsy produced by a blow on the head, in which great relief had been obtained by this practice. After applying the trephine the patient remained free from the paroxysm as long as the wound was open, though on the cicatrization of it the complaint returned.

Tissot says, there is a class of epilepsies which have their seat in the head, in the cranium, the investing membranes, or in the brain itself. In cases where external mischief may be considered as a cause of the evil, we ought not to hesitate, he

thinks, to divide the integuments so as to operate upon the bone itself; and he adduces several instances in which the operation of trepanning, which he recommends as perfectly safe, was attended with complete success. Epilepsies depending upon ossification of the membranes, or abscesses in them, or upon hydatids, schirrus, &c. he considers to be absolutely incurable.

Latterly, Baron Percy and M. Gondret have each published a work recommending the actual cautery in epilepsy. The former is in favour of using a very high temperature in the operation. He thinks that it gives less pain, and is more likely to be successful than a lower degree of heat. Instead of exposing the bone by an incision through the skin, he prefers burning through the integuments, not only because it is less painful, but also, because the "simultaneous cauterization of the skin, pericranium and the bone, excites in the cerebral system that specific commotion, that *sensation of fire*, which no other agent can produce."

"As the apparatus for cauterization," says Dr. Cooke, "is so terrific, Scullet has recommended a method of deceiving timid persons in the application; and Baron Percy informs us, that in cases of epilepsy, where the patients were weak and obstinate, he has taken advantage of the paroxysm to employ the cautery, which he has repeatedly done with advantage."

Our author has selected with great judgment, from other writers, all that appears to be useful in the treatment of epilepsy, in its various forms, and he has added, in his work, his own experience to that of others in favour of some of its remedies. We take leave of him with the assurance, that we were much interested in the perusal of his history and treatment of epilepsy, and that we think it calculated to be very serviceable, especially to the inexperienced practitioners of the healing art. T. M. H.

☞ Owing to an oversight on the part of the Editor, the heading of the first form of the above article was suffered to be worked off before its impropriety was detected.

ART. III.—*A Physiological Essay on Digestion.* By NATHAN R. SMITH, M. D. Professor of Anatomy and Physiology in the University of Vermont. New York. 1825. Octavo, pp. 94.

DIGESTION is a subject concerning which few of our readers will expect to hear any thing of a novel, or, perhaps, even of an interesting character. The peculiar properties of gastric liquor and bile, and the supposed means of chylification and assimilation have so often been detailed, in almost every imaginable shape, that the merest allusion to them is generally understood in the light of a truism. It is probable, therefore, that the title of the work now under review, will not prepossess the public with a very favourable opinion of its contents. We must confess that when we first entered upon a perusal of it, we did not anticipate much interesting matter; although, from our knowledge of the author's character and acquirements, we were certain of meeting with good logic and sound disquisition.

From the exposition, however, which we shall have occasion to make, of the novel and ingenious arguments advanced by Professor Smith, it will appear evident that his production deserves, and must eventually obtain, a much higher degree of estimation.

From the introductory chapter, which treats of the laws of physiological reasoning, we select the following paragraph—inasmuch as it alludes very distinctly to the leading doctrines of the author, at the same time that it will show the logical manner in which he is accustomed to reason.

"When we confine our inquiries to any particular science, with the laws of which we are acquainted, and reason from an effect to a cause, which in its operations we know to be controlled by those laws, there need be no fallacy. But if we enter another field of science, the laws of which we have not investigated, and reason from an effect to a cause, we shall almost certainly fall into an error, for we unconsciously attribute the effect to a cause, with the operations of which we are familiar. Thus, if we combine

two inorganic substances, and, after a time, observe that a remarkable change has taken place in their properties, we at once, and with propriety, attribute it to the influence of chemical affinities; for we know that such substances are exclusively under the control of physical properties. But if we convey certain substances into the stomach of an animal, a polypus for instance, and upon removing them after a considerable time, discover that they have become very much changed, having received new chemical and sensible properties, it is altogether unwarrantable to infer that these changes are wholly the effect of chemical influence; for, as the stomach of this and of every other animal is indued with certain vital properties, and as we are not yet fully acquainted with their laws or uniform habits of action, we cannot say how much, in this instance, is to be attributed to their influence.

"That similar effects, then, in any particular science, are referrible to the same cause, is generally true; but that similar effects in two distinct sciences, the cause being known in one, are referrible to the same cause, is, in principle, false."

As will be seen from the above extract, our author decides, *in limine*, against the doctrine of chemical influence, and attributes the whole process of digestion to the action of the living solids. In a chapter of "Preliminary Observations," he still further developes the same opinion, as preparatory to the more immediately argumentative part of his work. Under this head he takes occasion, after considering the phenomena of vegetable and animal life, to compare the digestion of animals with that of plants; and we must confess that he has made the analogy much more striking and illustrative than we have ever before imagined. As he enlarges very forcibly upon the same idea, however, in the latter part of the essay, we will pass on to an immediate consideration of his statements respecting the phenomena of digestion, and the theories which have been advanced in explanation of them.

In his summary view of the phenomena, which, as far as we can discover, is very accurate, our attention has been especially directed to the following particulars, inasmuch as he lays much stress upon them in his argument.—The accumulation of food in the stomach *immediately* appeases the sen-

sation of hunger, and in a short time restores the vigour and activity of the body, independently of the degree of distention which it affords.—The properties of the gastric juice are "very variable, or very obscure, since of the many experimenters who have carefully analyzed this fluid, scarce any two give the same results. Some have positively declared it to be acid, others, with equal confidence, have ascribed to it alkaline properties. A third class, discovering nothing remarkable in it, have presumed it to be nothing more than the residue of former digestions, mingled with saliva, and a similar secretion from the stomach."—The experiments of Montegre proved that the gastric liquor possesses no power of dissolving alimentary substances out of the stomach.—The properties of chyme are by no means uniform, as they vary with the food employed to such a degree, that there may be said to be as many kinds of chyme as of aliments.—If the eighth pair of nerves be divided, the process of digestion is immediately and completely suspended. The same effect is produced by violent affections of the mind and of the body.—In cases of sudden death, the food contained in the stomach does not continue to be converted chyme, although the stomach may be eroded.

We hardly need inform our readers, that the theory of solution by the salivary and gastric fluids, originally suggested by Chesselden, has been universally adopted by physiologists. According to our author, the erosions of the stomach first observed by John Hunter, and the experiments of Reaumur and Spallanzani, performed by introducing hollow balls, pierced with holes and filled with aliments, into the stomach, are the principal circumstances upon which this theory has been supported. The opposing results of Montegre and others, who have attempted to imitate the process of digestion, by means of the gastric fluid, out of the stomach, have not shaken the confidence of physiologists in the powers of this agent. Although the conversion of food into chyme is regarded as the most important part of digestion, still further

changes are supposed to be wrought on it by the action of the bile and pancreatic juice, after it has arrived in the cavity of the duodenum.

"Such is the theory of digestion at present universally received, and which has long since ceased to be questioned. So confident are men of science in its imperishable nature, that all our systems of physiology and pathology are founded upon it as their corner stone. We are to recollect, however, that the above experiments and deductions were made at a time when the rules of logic, by which we are guided in speculations of this kind, were less rigid than they now are. We are to consider, also, that at that time, the progress of science had shown the previous theories to be altogether inadequate to the explanation of the phenomena of digestion; that the application of chemistry in its improved state to physiological inquiries, was then universal; that, consequently, the ingenious theory in question, offered as it was, by men of high character and great influence, soon came to be generally received, and consequently, ceased to be the subject of that spirited inquiry, which results from difference of opinion.

"But are the evidences in favour of digestion, by the solvent powers of the gastric fluid, sufficient to check further inquiry on this subject? Is the mode of reasoning, which has led to this opinion, conformable to just principles of logic? Does the theory, like that of Harvey, rest upon numerous and unequivocal facts? Are the phenomena of digestion satisfactorily explained by the supposition, and does it harmonize with the general anatomy, physiology, and pathology, of the system."

Our author objects to the mode of reasoning employed in support of this theory, as one which has been "productive of infinite perplexity and error, and which has been uniformly found inadequate to the explanation of physiological phenomena." The first abettors of the doctrine "were led to anticipate the existence of a powerful solvent in the stomach, from comparing the changes which the food undergoes in that organ, with results obtained in their laboratories. This was evidently predicting the means by which nature should accomplish her ends in one department of science, by observing what she has done in another, in which the properties of matter, and the laws by which they are exerted, are entirely different."

"Were it impossible that the changes effected on the aliment in the stomach, should be accomplished in any other way than by a mechanical or

chemical process, their conclusion would be just, for there could be no fallacy in reasoning from an effect to a cause, the operations of which we are acquainted with. But knowing the distinction which exists between the physical and physiological sciences, it is not only possible, but even probable, that this change is wrought by a cause, the operations of which we are but little acquainted with. It is incumbent on those, therefore, who maintain the above position, not merely to show the effect, but to demonstrate, unequivocally, that the gastric fluid possesses such properties as are capable of producing the effects attributed to it, or to show that they can be accomplished in no other way.

"The chemical powers attributed to the gastric fluid are certainly very extraordinary. It is said not merely to dissolve rapidly the great variety of aliments which are taken into the stomach, but to enter into combination with them, and to produce a third substance. Should we not, therefore, expect to discover something equally extraordinary in the *properties* of so powerful an agent? But I have already stated, that this is so far from being the case, that scarce any two persons give the same account of its chemical constitution. Its analysis, therefore, yields nothing to support the theory in question."

We have already alluded to the two principal facts upon which the theory of digestion, by the gastric liquor, is chiefly supported—the erosion of the coats of the stomach after death, and the digestion of food contained in hollow balls, and introduced into the stomach. The author attempts to explain away their force by the following ingenious reasoning:

"With regard to the first of these phenomena, it appears to me, that the advocates of the theory of solution are not aware of the dilemma to which they are reduced, in case it be admitted as an effect of the gastric juice. Magendie, in accounting for the inertness of the gastric juice out of the stomach, attributes it to the absence of certain circumstances, which cannot be accurately imitated. The principal of these are uniform heat and motion. They, it is obvious, are dependent upon vitality, and must cease with it. If, therefore, it be asserted by any, that this fluid acts thus upon the dead stomach, with an energy equal to that of the most powerful acids, it is certainly incumbent on them to exhibit its effects out of that organ, since there can be no reason why they should not be unequivocally demonstrable. If the negative argument be resorted to, that these effects can be produced in no other way, I consider the assertion to be altogether unwarrantable, and the mode of reasoning to be, on physiological subjects, altogether inadmissible. The changes which take place in the human body, in the article of death, have often been observed with astonishment. So numerous and so

striking are they, that much caution is required to distinguish them from those changes which are the result of disease. We know very well, that if we would discover, after death, the morbid evidences of a slight degree of inflammation, the examination must be promptly made, or those evidences disappear. We know, too, that absorption is performed with great energy in the article of death, and I believe after apparent death, or after the animal functions have ceased to be performed. We have reason, also, to think, that appearances, resembling those of ulceration, have taken place after apparent death. These *post mortem* changes are, in a general way, accounted for, on the supposition that the organic functions, particularly those of the capillaries and absorbents, continue to be performed after the animal functions have ceased. Who, therefore, will undertake to say, that the abovementioned erosions of the stomach are effected by a fluid, secreted by that organ, and possessing remarkable chemical properties, because it is impossible that they can be accomplished in any other way? We know very well, the extreme vascularity of the stomach, and that its capillary vessels, and particularly its absorbents, act with unparallelled energy. We know also, that different portions of the stomach derive their vitality from different portions of the nervous system. Is it not possible, therefore, to conceive that one part of the stomach, having lost its vitality, may be acted upon by another which retains it? It is by no means incumbent upon me to make out, even the probability of this supposition, the bare possibility of it is obviously sufficient for my purpose.

"Another objection to the common explanation of this phenomenon is, that in all those instances in which it has been observed, the food, taken into the organ a short time previous to death, has been found untouched, however extensive the erosion of the stomach may have been. It is certainly very singular, that this fluid should produce no effect on the aliment, which, to prepare it for its action, has been comminuted by the teeth, mixed with the saliva, macerated by the fluids of the stomach, and finally imbued with the gastric juice itself; and that it should turn with energy upon the stomach, which has been influenced by none of those circumstances, and which is moreover protected by an epithelium, or cuticle, and its own mucous secretions.

"The experiment of introducing hollow balls, or tubes filled with aliment, into the stomach, was first performed with a view to disprove the supposition of the mechanical action of the walls of the stomach. In this respect, it has completely succeeded; but it has been also regarded as unequivocal evidence of digestion by the gastric juice, although it by no means suggested that theory. Let us candidly examine the circumstances of this experiment, and see whether the above inference necessarily follows. The balls or tubes, employed for this purpose, must have been small, and pierced with holes, either numerous or large, in order to favour the free access of the gastric juice. On the introduction of the balls into the stomach, the

progress of the experiment is entirely hidden from our view, and we know nothing of it, but from the result, which on removal of the balls, is found to be a partial digestion of the aliment contained in them. If it can be shown, then, that it is impossible, that the phenomenon can be accomplished in any other way, the conclusion is just; but this, as in the instance above, is obviously absurd. I hope to make it appear not only possible, but even probable, that the villi of the stomach act immediately on the aliment, and effect the changes that have been attributed to the gastric juice. It is enough for my present purpose that such a thing is obviously possible, and being so, the phenomenon in question is no longer proof of the solvent powers of the gastric juice; for the villi of the stomach, being probably capable of elongation, and attached to a very moveable membrane, would be capable of seeking the aliment contained in the stomach, and of coming in contact with that which is inclosed in the balls; and it is to be observed that it is only that portion which is in the neighbourhood of the holes, that is found changed.

"It is proved by Wilson Philip and others, that it is only the surface of the alimentary mass, in contact with the walls of the stomach, that is changed by the gastric juice: therefore, only that part of the aliment in the balls, which is reached by the surface of the stomach, can be acted upon, according to either supposition. Hence, the fact is as much in favour of the one position as the other, and proves nothing.

"A fact, to which I have already alluded, calculated to throw much light upon this obscure subject, is that mentioned by Philip, and also by Magendie. They both state that, in digestion, no change takes place in the alimentary mass, (except such as maceration effects,) until certain motions are observed to take place in the stomach, by which its inner surface is closely applied to the surface of the aliment; that then, the first changes which are discovered, are on the surface of the food in contact with the mucous membrane of the stomach. A thin layer of aliment is found to be converted into the substance, which is called chyme. This is then conveyed, by the motions of the stomach, to the pyloric extremity of the organ, and a new surface is presented to the surface of the stomach to undergo the same change. To those who advocate the theory of solution, the above must oppose no small difficulty. The fluid, which, we are told, begins to be poured out soon after the food enters the stomach, and which, in some instances, is found in considerable quantity in that organ, must certainly pervade the whole mass of the aliment; why then should its effects be confined to the surface of the alimentary mass? and why should they be simultaneous with certain motions of the stomach? It may be said, indeed, that this fluid acts with such surprising energy, that, at the instant of its secretion, it seizes on the first particle of food with which it comes in contact, and that thus it never permeates the mass of food. This resort, however, would, perhaps, cost our opponents more than they are aware.

It would amount to an absolute denial of the action of the gastric juice upon the dead stomach: for how could it act upon that organ after death, unless it were accumulated in it, and that, when there is food present upon which it has not acted? It would also follow from this supposition, that the secretion of the gastric juice does not commence until the very moment that digestion is found to take place. We might also fairly infer, that its secretion would never take place except food were present, and that then it would never remain uncombined in the stomach for a moment. It would, therefore, be utterly impossible to prove the existence of such a fluid, and in advocating it, the abettors of the theory of solution would abandon all the arguments usually employed, and gain nothing but a supposition.

"Those who believe that the gastric fluid can only act when aided by certain vital powers residing in the walls of the stomach, and account in this way for digestion taking place only on the surface of the mass, in contact with the stomach, are in the same dilemma. On such a supposition, there could be no more proof adduced of the existence of the gastric juice than of the existence of the nervous fluid of which so much was once said. We see, indeed, the surface of the aliment coming in contact with the surface of the stomach, and a remarkable change effected upon it: but shall we say, that this is effected by a fluid possessing remarkable chemical properties poured out at that instant and enabled to act by the vital powers of the stomach? If its powers are exerted in no other way, and at no other time, what evidence have our senses, that there exists any such fluid, except the effect that is produced upon the aliment? I have already shown that this reasoning would be inadmissible.

"The promptitude with which digestion is interrupted, by the division of the eighth pair of nerves, is a sufficient evidence that there is something more than a mere chemical process going on in the stomach, for, according to the commonly received hypothesis, the gastric juice, already secreted, should continue to operate upon the aliment."

In addition to these objections, so ingeniously advanced against the commonly received theory, the author resorts to several other arguments, which are less immediately connected with the usual course of reasoning upon this subject. He insists that, as changes of composition are never wrought upon *volumes* of matter in any of the large reservoirs of the system, it is unreasonable to suppose that digestion can be performed by any influence directly exerted, either by the stomach or its secretions, upon the whole mass of the ingesta.—He intimates, that the variable properties of chyme, now universally acknowledged by physiologists, and the fact that chyle

has never been found in the intestines, both stand opposed to the theory of solution by the gastric juice.—He also thinks that the same theory "does not satisfactorily account for the speedy restoration of strength and vigour, which one experiences on taking food who has fasted till weak and faint." It would appear that some portion of the food must immediately enter the circulation, in order to produce these effects; while, according to the common opinion, the whole period of digestion is consumed in imparting to the ingesta, instead of receiving from them.

The anatomical objections of our author are very striking, and appear to be entitled to consideration. He renders it evident, that the uses of several of the structures concerned in digestion cannot be explained on the commonly received theory, so well as by the one which he has proposed in the latter part of his publication. For instance, the villi of the stomach are apparently organized exactly in the same manner as those of the small intestines. As similarity of structure in every other situation indicates similarity of function, it would be philosophical to attribute the same properties to both kinds of villi; nevertheless, as our author observes, "we are told that their offices are diametrically opposite, that of the villi of the stomach being to exhale the gastric juice, while those of the intestines absorb the chyle." The anatomy of the *thoracic duct* is also shown, in a very beautiful and explicit manner, to be altogether inconsistent with the idea that all the nutritive particles eliminated from the alimentary mass, gain access by it alone into the circulation. He also insinuates that the functions attributed to the liver, by the present theory of digestion "do not correspond with its magnitude, its structure, and its pathological importance."

Having given this very cursory view of the arguments dwelt upon much more largely by our author, we pass on to the detail of *his* theory, entitled "*digestion by the veins of the stomach and intestines, and by the liver.*" As far as we can discover, it is altogether novel; respecting the inge-

nuity of it, our readers must judge from the following extract, and from the reasoning subequently advanced:

"In the foregoing chapter it appears, that no important change is effected on the aliment until it comes in contact with the mucous membrane of the stomach. I trust that I have also shown the existence of a gastric liquor possessing the powers so long attributed to it, to be a fallacy. As I have before stated, we see the aliment come in contact with the walls of the stomach, and an important change immediately taking place upon it. If I understand the relation between cause and effect, we are not to interpose a new link in the chain of sequents, for the sake of aiding the imagination, or for completing our preconceived notions. The presumption is, then, until some new intervening cause can be pointed out, that the change in question is effected by the immediate agency of the mucous membrane of the stomach. If we go beyond this, and assert that the change is effected by a particular portion, or set of vessels, contained in this organ, we require further proof. My proposition is, that the first step in the process of digestion is effected by capillary veins, originating in the villi of the stomach with absorbing extremities, and terminating in the great branches of the vena portæ. The arguments in favour of it are drawn from the results of certain experiments, from the nature of the change effected on the aliment, from the analogy of plants with animals, and from the anatomy and pathology of the digestive organs."

Our author next devotes a long chapter to a consideration of the doctrine of venous absorption, which now appears to be universally regarded as established beyond all further dispute. In very forcible language he demands of those who are so fastidious as still to doubt the truth of this doctrine, to produce equally strong evidence that the lymphatics do actually perform the function so long attributed to them. He would not, however, be understood to deny the functions which the lymphatic and lacteal absorbents are supposed to perform. But he seems to believe that the lymphatics perform some other office than that of removing effete and excrementitious matters from the system, inasmuch as their contents consist of a fluid which is uniform in its sensible and chemical properties, and is highly animalized like the constituent principles of the blood. As his opinion upon this subject appears

to be original as well as interesting, we will transcribe it at full length.

"If the lymphatics exert no influence on their contents, but exist merely for the purpose of taking up that which has become useless, and conveying it into the mass of the blood, why, since nature accomplishes her ends by the simplest means, do not the absorbent branches terminate in the nearest veins, instead of traversing so great a distance to reach the left subclavian?

"From the length of these vessels, which is greatly increased by the convolutions which probably take place in the lymphatic glands, from the nature of their contents and other circumstances, we are led to inquire whether they are not concerned in the important process of animalization, receiving certain parts of the blood from the capillary arteries, which they exalt to a higher degree of organic life. The manner in which their contents combine with the chyle in the thoracic duct, harmonises with this supposition; for we may rationally suppose that they exert an influence on the partially animalized, though comparatively crude fluid, introduced by the lacteals. By the theory at present received, the only fluid that nourishes the system, is here mingled with the refuse matters."

In regard to the kinds of substances which the veins of the stomach are capable of absorbing, he conceives that no more difficulty need be connected with solids than with fluids. Medicinal substances of an insoluble nature, are generally believed to be taken into the circulation. The bones, and other solid parts of the system, are, also, sometimes taken up with surprising rapidity. "In some instances the most intractable substances, when lodged in the flesh, have been absorbed and removed. Plants, by their roots, decompose soils on which they feed, and take up substances which are the most insoluble in nature." Whether solid substances are taken up by the veins in a state of extreme comminution, or whether they are previously resolved, by the powers of life, into their ultimate principles, our author does not undertake to determine.

The appearances of the chyme are considered in the following paragraph, which cannot fail to interest our readers:

"The sensible properties of the chyme I have already mentioned, as furnishing arguments against the opinion in question. They are not less im-

portant in the support which they yield our hypothesis. On the supposition that the villi of the stomach act upon the aliment as suggested above, electing from it such principles as are subservient to nutrition, the appearances of the chyme are precisely what we should expect. The sensible properties of chyme suggested to some of the older anatomists the idea that the aliment was, in the stomach, attacked by a myriad of small worms which reduced it to the uniform pulpy mass of this substance.*

"That this may be effected by a myriad of absorbing mouths, the changes which take place in the aliment render not improbable. An extremely minute mechanical division would of course be effected upon it, and perhaps many substances would be decomposed, some of their proximate principles being taken up. A change in the sensible and chemical properties equivalent to what we actually discover, would of course take place. We should also expect that the chyme would be variable in its appearance as the food employed varies.

"The manner in which different portions of aliment are successively changed, is also much more satisfactorily explained by our hypothesis, for that only can be acted upon which is in contact with the surface of the stomach. This, as stated by W. Philip, by the peculiar action of the muscular coat of the stomach, is made to give place to another layer, and the digested portion is conveyed to the pyloric extremity. The appropriate motions of the stomach are constantly evolving the central parts of the mass and bringing them in contact with the surface."

As Bichat long ago observed, the organic functions of animals and vegetables are of a similar nature. Analogies may therefore be drawn between the two kingdoms, without any violation of the strictest rules of logic, provided the comparison be confined to those properties which are common to both. Of this opportunity the author has taken full advantage, as will be seen from the following extract:

"One of the characteristic differences between plants and animals, is, that the former are fixed to the earth by their roots, the radicles of which take from the soil whatever affords nutriment to the plant, and convey it immediately into the circulation, where it becomes assimilated to the dif-

* "Perhaps they were as near the truth as those who consider the process to be performed by a chemical agent, for neither the existence of these wonderful worms, nor of the not less wonderful fluid, can be proved, and if we reason from the effect alone, we should with more propriety ascribe it to the former."

ferent textures of the plant. Animals, on the other hand, must have locomotion, and to render this consistent with digestion, the animal is furnished with an internal cavity capable of receiving those substances which yield aliment, though in a much more concentrated state than they are presented to the roots of plants.

"Plants, it has been often observed, do, in their general structure, as well as in their organic properties, bear a rude resemblance to animals. They have a cuticle, a cutis, and a skeleton. Their sap vessels too, perform a circulation more simple, indeed, but not unlike that of the blood-vessels of animals, and their leaves are obviously their lungs, in which their blood is attenuated and exposed to the action of the air. They have also their secernents and exhalents. But to what in the animal economy shall we compare the radicles of vegetables? I think it will appear that there is no forced resemblance between them and the villi of the stomach. The roots of plants, indeed, are of comparatively great length, and necessarily so, for plants having no prehensive apparatus by which their proper aliment is elected and brought in contact with their roots, the latter must go in quest of it, ramifying to a considerable extent from the stalk of the plant, and drawing nourishment from a great volume of soil. Hence, also, the roots of plants are constantly elongating and multiplying. The radicles of vegetables are indefinitely small and numerous, much more so than one would imagine, who has merely examined such as are torn from the earth. If they are raised with a large volume of earth and this be carefully washed away, they will be found as minute and numerous as the villi of the stomach. The *radicles* of the stomach are very differently circumstanced. The substances from which they are to elect food, are in a much more concentrated state, conveyed into the cavity of the organ, and by its muscular motions successively applied to its surface. Yet they probably have, in some degree, the power of seeking their food by elongating themselves, and by the mobility of the surface to which they are attached.

"By the prevailing theory of digestion, however, an action, by no means analogous to that of the radicles of plants, is attributed to the villi of the stomach, for these last are supposed merely to exhale a fluid which acts chemically upon the contents of the organ. Thus too the digestion of an animal must be less under the control of vital or organic properties than that of the humblest plant; for in the latter, nothing appears to be left to the agency of physical properties, but all to be accomplished by the vital or organic powers. The radicles of plants are endued with the surprising power of acting upon the most intractible substances and conveying them into the circulation; thus silex is detected in the textures of some plants.

"We should expect, in rising from the lower to the higher grades of existence, to find the functions more purely and exclusively under the control of those laws which constitute life, but if the above theory be true, the

laws of inorganic matter have, so far as we can ascertain, more influence over the animal than the vegetable functions. Is it not then more rational to infer from the analogy of structure which I have pointed out, a corresponding one of action, and that the villi of the stomach are endued with the power of electing and absorbing alimentary substances?"

The digestion of some of the inferior classes of animals is also made to bear very strongly in favour of the new theory, particularly that class, the individuals of which do not possess any internal cavity, but take up their nourishment by a kind of placental surface. "Does any one believe," asks our author, "that they perform digestion by means of a gastric juice?" Under this head the case of a fish, which had partly swallowed a large one of its own species, is detailed. On examining that part of the swallowed fish which had been lodged in the cavity of the stomach, it appeared as if a considerable portion of its substance had been eaten away, while that which had not been in contact with the living surfaces remained altogether unchanged. In the stomach was nothing like chyme or chyle, but merely a frothy mucous. Such appearances, in the opinion of our author, correspond better with the theory of direct absorption, than with that of a solution by the chemical action of any fluid.

The following extract will exhibit the new theory in another interesting point of view:

"But I fancy that my opponents are impatient to inquire of me in what manner the changes of assimilation are wrought, and what apparatus is appropriated to that function, in case the process is so little effected in the cavity of the stomach. I reply, that it is accomplished in the capillary and other vessels, through which the aliment is made to pass before it is added to the circulating mass of the blood.

"It is obvious that whatever is absorbed by the veins of the stomach, will be first conveyed through the tissue of vessels indefinitely minute, which may be said to compose the villi of the organ; next through the branches which ramify in the cellular coat, through the long tortuous vessels of sensible magnitude, meandering upon the surface of the stomach and between the laminæ of the peritoneum.

"This, however, is not the most interesting part of its course. On reaching the trunk of the vena portarum, along which it is slowly propelled, probably by the action of that vessel, it is conveyed to the great fissure of the

liver, to be diffused through that large and important viscus, having first been mingled with the returning blood of the spleen, the pancreas, the omentum, &c. It is then again subjected to the action of a congeries of capillaries constituting the substance of the liver.

"Here, then, is an ample opportunity for all those changes to be wrought, which may be deemed necessary for the conversion of aliment into blood. This supposition too, accords well with what we observe in the performance of other functions, by which changes are wrought in the properties of circulating fluids, viz. that they are accomplished not upon large volumes and in large reservoirs, but upon attenuated streams and in minute vessels.

"I have heretofore objected to the prevailing theory, that it does not appreciate the importance of the liver nor attribute to it an office corresponding to its magnitude, structure, and pathological consequence. The hypothesis which we offer exalts it to the highest degree of physiological importance, regarding it as, perhaps, the most efficient organ concerned in animalization."

The phenomena of the fœtal circulation are brought forward by our author, to confirm his peculiar views; and as he has drawn a beautiful comparison between the changes which take place in this circulation, and in that of the lungs, at the time of birth, we shall make no apology for occupying our pages with another long extract.

"It is well known that previous to birth the liver predominates over all the other viscera. It is undoubtedly thus early developed for important purposes, performing, at that time, some indispensable office in the fœtal system. It is conjectured by some that at that time it exerts an influence on the blood which is subsequently assumed by the lungs. The blood which is received from the maternal system is conveyed to the liver by the umbilical vein. It there enters the portal sinus, a part of it being taken up by the ductus venosus, while a large proportion is conveyed by the ramifications of the vena portarum through the glandular structure of this organ. It is not in vain that the fœtal blood is thus circulated through the liver, it is undoubtedly for the purpose of its being wrought upon by the capillaries of that gland, and it is for this purpose that the liver is thus early perfected.

"In the fœtal state the system of the vena portæ is organized at the same time with the digestive organs, but, like the vascular system of the lungs, it is in a great degree dormant, the quantity of blood which it conveys being merely sufficient for their nutrition. The moment, however, that di-

gestion begins to take place after birth, these organs become centres of fluxion, and the vena portarum becomes charged with blood possessing peculiar properties, which, in some way or other, it receives in the stomach, intestines, &c.

"At birth the circulation by the umbilical vein immediately ceases, and its place, as soon as digestion commences, is supplied by that of the blood of the vena portarum. Must we not infer, then, that this last is circulated through the liver for the same purposes as the former? for it is conveyed in the same vessels, and we cannot suppose that they are at once endued with a new action, entirely different from that which they had before performed and which is merely for the purpose of furnishing bile, a fluid which is not needed in the fœtal state, as no digestion then takes place.

"Here, then, as well as in the lungs, we see a surprising change taking place in the circulation at the time of birth, not so promptly, indeed, as that of the lungs, nor so immediately essential to the continuance of life, but not less so in its ultimate effects. We have every reason to think that, as is the case often in the lungs, so also in the portal system an imperfect establishment of its circulation, either from organic defect in the liver or some other organ, is often the cause of infantile diseases marked with indigestion and imperfect assimilation and which often terminate fatally. Who, that has been engaged in extensive practice, has not witnessed the frequent occurrence of infantile jaundice, and that too taking place as soon as digestion has commenced, and in children born apparently healthy."

Here follows another comparison, which is also made to bear strongly in favour of his theory.

"In conclusion of this section, I would point out an interesting analogy, which follows from our hypothesis, between what may be called digestion in the fœtal state, and digestion after birth. It is pretty well ascertained, and if I mistake not, generally believed, that the fœtus must be nourished from the blood of the mother, by means of the absorbing powers of the capillary veins in which the umbilical vein terminates, and which appear to constitute a considerable part of the fœtal portion of the placenta. By our hypothesis the function at birth, is transferred to the capillary veins of the vena portæ, which, as above, appears to perform precisely the same office in relation to the liver, that the umbilical vein does before birth. Let it be carefully noticed too, that *they are the opposite extremes of the same continuous vessel.*

"Another circumstance which strengthens the analogy between the nourishment of the fœtus and digestion after birth, is, that as a part of the nutriment in the latter case is, without circulating through the liver, as I shall by and by more particularly state, conveyed by the thoracic duct into the

venous blood, to be circulated through the lungs before it serves the purposes of nutrition; so also in the former, a portion of the fœtal blood, by the ductus venosus, immediately enters the heart, and as it has in the placenta undergone that change which after birth is effected by the lungs, it is immediately fit for nutrition, and is distributed for that purpose.

"In considering this subject, we are very naturally led to inquire whether the blood of the vena portæ exhibits any properties which would favour the belief that the veins of the stomach and intestines do largely absorb alimentary substances. The unbiassed observations of many, furnish us with a satisfactory reply. Dr. Rush, who quotes authorities in support of the assertion, tells us that the blood of the vena portæ is more crude than the blood of other veins, that it coagulates more slowly, and that it undergoes putrefaction less readily; circumstances which decidedly indicate a less degree of animalization. I believe it is commonly thought by physiologists, that the blood of this system differs more from arterial blood than that of other veins."

The experiments of Flandrin, performed on the branches of the vena portæ of horses, are referred to by the author with a strong degree of confidence. The observation of Haller, respecting the presence of oil in the blood of the same vessels, is also alluded to in relation to this point. The professor was so struck with the importance of these circumstances, that he expresses himself, as really surprised that they "did not suggest the hypothesis," which he thinks he "may say without vanity, we have found at least very plausible upon other grounds."

Hitherto we have found no occasion to object very seriously to the reasoning of our author in regard to any circumstance; in his concluding chapter, entitled "Objections to the Hypothesis of Absorption," however, we have encountered some ideas which we cannot pass over without animadversion. To anticipate an objection which he seems to fear will be brought against his theory from the want of predominance in the size of the united calibres of the veins of the stomach over that of all its arteries, he asserts that the "arteries of the stomach during digestion are exhaling as great a volume of fluid as is absorbed by the veins." A little further on he shows some concern, also, lest the weight of the whole of the

food taken in should not be found to overbalance that of the digested chyme; and he attempts to foreclose whatever objections may be drawn from such a fact, by stating that "the volume and weight of the food are so much increased by the exhaled fluids, that the removal of its more nutritive parts might not be appreciable." Had these assertions proceeded from a mind of an unreflecting character, or had they been made by almost any other person than the author of the ingenious essay which we are now analyzing, we should not, perhaps, have been so much surprised at their fallacy. As it is, they stand manifestly in discordance with the theory which they were intended to support. Our readers, however, will experience no difficulty in ascertaining the position which they must occupy in regard to this subject. Exhalations are constantly taking place from every part of the surfaces, and the secretions are as constantly oozing out through almost every outlet of the body. To supply this incessant wasting, new materials must be somewhere introduced into the circulation. Now the mucous membrane of the digestive apparatus is universally acknowledged to be the only avenue through which any appreciable amount of nutritive particles can possibly gain access to the system. If we suppose, for a moment, that the exhalation from this surface can be equal in quantity to the materials absorbed, we immediately cut off every means of supply for growth and renovation, and the body would, therefore, be undergoing, in all circumstances, a regular disorganization of its materials.

But we cannot divine what could have induced Professor Smith to anticipate any thing contradictory to his theory from the result of experiments, however carefully made, upon the comparative weight of food before and after digestion. For our parts, we are persuaded that the quantity of all the ingesta, both fluid and solid, will be found to be constantly diminishing from the upper to the very inferior regions of the digestive tube.

To prevent the cases of malformation of the liver, related by Lawrence and Abernethy, from being brought forward against his theory, our author offers an explanation which he thinks will give additional weight to his opinions. The hepatic artery was, in those cases, much larger than usual, and it is, therefore, "obvious that the whole mass of blood must have been, in a short time, subjected to the action of the liver." The great peculiarity was, that "the aliment must first have mingled with the mass of the blood after having undergone the action of the absorbing veins of the stomach and intestines, before it was acted upon by the liver." He intimates that these cases resemble those of malformation of the heart, in which life is for a long time supported, although but a small proportion of blood passes through the lungs. But these are not the sentiments at which we were disposed to take exception. He speaks of the influence of the liver upon the circulating blood, as if the secretion of bile were not an office necessarily connected with a performance of its healthy functions. The spleen and pancreas, according to this idea, may, under certain circumstances, "vicariously assist the liver" in assimilating the aliment to perfect blood. In another part of the "essay," he thinks he has "shown that the secretion of bile is probably effected from the arterial blood of the liver."

To both of these opinions, a very little reflection on the part of our readers will afford strong objections. The liver is a real gland, possessing all the parts of that organization which in other glandular structures appears to be specifically calculated for the separation of a peculiar fluid from the blood. The state of its secretion regulates that of the general health; and just in proportion to the derangement of this secretion are the whole digestive and nervous systems thrown into disorder. According to our author's own logic, therefore, it would have been more philosophical to have considered the formation of bile as the especial function of the liver, without

which none other connected with that organ can be performed in a regular or salutary manner. We should have a poor opinion of the spleen and pancreas as subsidiary organs to the liver, provided the bile could not be made to flow from any of the parts concerned in digestion. Nor are we willing to think more highly of the importance of the hepatic artery on account of the credit allowed to it by our author. Because a considerable quantity of lightly coloured fluid, remotely resembling bile, was found in the gall-bladders of the two infants which were found by Messrs. Lawrence and Abernethy to present malformations of the liver, it does not follow that healthy bile can be secreted by the hepatic artery in well formed subjects. On the contrary, we should suppose that the want of health, and the early death of those children, as well as the fact that no instance of a similar malformation has ever been found in subjects of a greater age, would all be considered unfavourable to such a supposition. The insignificant size of the artery, when compared to that of the liver and its excretory duct, and the immense quantity of bile which, under some circumstances, is suddenly secreted, might also be brought forward in opposition to the same idea. But it is unnecessary to say more in relation to this subject: the very mentioning of the only few objections which we have found occasion to make against the reasoning of Professor Smith, will excite the attention of our readers sufficiently for our purpose.

Before we close this analysis, we will notice one difficulty which, no doubt, has already been started in the minds of our readers. What will the author do with the lacteals? Is there no use to be attributed to them in the economy of digestion? The professor has undertaken to answer these questions for himself; and we, therefore, dare not presume to do it for him.

"I presume that it will be inferred, from what has already been said, that I by no means deny *in toto* the functions attributed to the lacteals. Their office is, undoubtedly, to take up a portion of the aliment, to subject it to

their own action and that of the mesenteric glands, and finally to pour it into the mass of the blood. They probably absorb that part of our food which is capable of being converted into some one of the constituent parts of the blood, without undergoing the action of the liver. What part of this fluid it is which is furnished by the lacteals, and what by the liver, I am unable to say. It has been supposed by some, that the latter organ was concerned in elaborating the red globules. That it assists in completing some one or more of the constituent parts of the blood, I think will follow from what has been said."

Notwithstanding the exceptions which we trust we have candidly made to some of the minor points above alluded to, and the guarded manner in which we may have occasionally appeared to express our own opinions, our readers must have perceived that we hold this production of Dr. Smith in very high estimation. Whether the theory which it proposes will eventually be adopted by the profession, we cannot venture to determine; of this, however, we are certain, that the sound and logical reasoning which it contains, and the novel and interesting views with which it abounds, will recommend it to the notice of every philosophical inquirer into the mysteteries of our science. M.

ART IV.—*A Nosological Practice of Physic, embracing Physiology.* By GEORGE PEARSON DAWSON, M. D. London, September, 1824.

AMONG plain honest republicans, it is not to be expected that much regard will be paid to the titles of books, any more than to those of men. We know too well that the former, as well as the latter, however high sounding, are too often far from being indicative of the value of the things to which they are appended, and that in many instances, they deceive those who form their judgments from them, into lofty notions of a worthless work, in the one case, and into a servile admiration of an empty head in the other. To neither, therefore, do we pay the smallest deference, though we are aware that both are passports to the respect of many good folk on this, and of many more on the other side of the water. To such the title of

the work now before us, "a Nosological Practice of Physic, including Physiology," would have been a promise of something rich, original, and interesting. To us, for the reasons just stated, it was *vox et præterea nihil.* We built no opinion of the probable character of the volume upon it, but passed immediately to an examination of its contents.

The advertisement on which we first cast our eyes gave us at least a hope that the time spent in a perusal would not be entirely wasted; for it informed us that the work was "the result of twenty-eight years devoted to the study and practice of medicine—that the object the author had in contemplation was to exhibit general views of the principles and treatment of diseases, interspersed with *classical*, *physiological*, and *pathological* facts and observations, and that he had constructed a system of nosology for himself, simple, intelligible; and consentaneous with his own experience." This we conceived to be a plain statement of the pretensions of the work to public consideration, and with this to guide our anticipations we travelled from its commencement to its termination—with what satisfaction, it is the purpose of the present article to inform the readers of the Medical Review. The Nosological arrangement, "constructed for the *author's own use,*" presents itself to our consideration first:—Diseases are divided into five orders, viz. 1st, Febrile; 2nd, Inflammatory; 3rd, Nervous; 4th, Cachectic; and 5th, Functional. To each of these belongs one genus, under which the several species treated of, are arranged; that of the 1st, including eleven; of the 2nd, thirty-one; of the 3rd, twenty; of the 4th, twenty-two; and of the 5th, twenty. Even to name all these would carry our paper beyond its proper limits; we shall therefore not venture on the presentation of any thing more than desultory sketches of such parts as have attracted our attention, either by the peculiarity of their style, their singularity, or their special excellence or worthlessness.

On the two first species, of the first order, we have found nothing worthy of serious comment. The latter, febris remittens, is divided into febris mitis and febris flava, the term flava

meaning, as we presume, neither more nor less than maligna, in contradistinction to mitis. We are indeed led to this interpretation by the proposed treatment of it; bleeding to the amount of fifty ounces, being, in the opinion of the author, *never* wrong. To this sweeping conclusion he adds, that "emetics must be interdicted in *every* case, as in the opinion of Moseley, they make the patient vomit away his life." In the next sentence, also, Dr. Moseley is again introduced to inform the reader, that "negroes bear amputation without a murmur." What this has to do with febris flava, any more than with cera flava, we cannot imagine, unless it is meant to justify bloodletting in fifty ounce quantities, by showing, that all the vessels of an entire limb may be divided, without fear of hurting the feelings of the patient. Under the head of Remittent Fever, we meet with a specimen of the *classical*, which we transcribe for our readers as exceedingly bathetic. "The Roman land, once the seat of warriors, statesmen and orators, trodden by the *Fabiuses*, the Scipios, Julius Cæsar, Cicero, Sallust, Livy and Tacitus, appears still to suffer nearly as much from marsh miasmata as it did during the days of Baglivi." A little further on we have also something in the shape of a bull for our amusement. We are informed that "to sleep a night at Baccuno is fatal: by doing so, an English physician lost his life and his wife *escaped*, (*i. e.* by doing so too,) after a dangerous illness."—The sections headed Hectic, Synocha, and Synochus, are filled with common place remarks, not even calculated to amuse. Under the head of Typhus, the author sagely informs us that "contagion is all a mystery, and like all other mysteries will long remain without an Œdipus." He then contends stoutly against the doctrine of spasm and debility in fevers generally, commencing with his postulate thus: "With respect to spasm—far famed spasm, which some men cling to, as a mother does to a sickly child, or a wanton to her seducer, it *never* exists, and *cannot* do so." The arguments which follow, though collected during twenty-eight years experience, have not succeeded in moving us either to the right hand

or to the left of the subject in dispute, and shall therefore be left within the lids of the octavo before us. The reasonings on congestion, to be found in this part of the work, may also be left undisturbed without the risk of any serious disaster befalling the profession, for lack of them. The *treatment* recommended in typhus fever, consists in "*prompt* and *copious venesection*, repeated purgatives, &c." "Tempora mutantur, et nos," &c. On the remaining species of the first order, we refrain from offering any observations, lest the honesty which would compel us to say, we found nothing interesting under any of them, might be mistaken for captiousness.

The 2nd order is somewhat more prolific than the one just noticed, in classical, physiological, and pathological observations. The following specimens are fairly illustrative of the character of each. Under the head of Opthalamia, the author impresses upon us the conviction that without eyes no man can see, and informs us that Leonardo da Vinci thought one was better than two to view objects at a distance. He also states, on the authority of Mr. Cline, that "Dr. William Hunter *imagined* himself to look through the window at the passengers in the street, which he performed without the least uneasiness; next he *resolved* to view the distant Surry hills,—he did so, and the pain was so intense as to make him instantaneously cover his eyes with his hand." Who can learn from the language of the author here quoted, whether Dr. H. did really look at the passengers in the street, or whether it is meant, that he merely *performed his imagination?* Whether he really did see the distant Surry hills, or whether he barely resolved to do so? This, like "contagion and most other mysteries, will, we fear, be long without an Œdipus." Under Parotitis we have another spice of physiology, for the author deems it a proper situation to enter on the consideration of the female breasts. He remarks that "the breasts,—most sacred fountains, as Blumenbach observes, and as Gellius Favorinus, the philosopher, elegantly calls them, the 'Educatores' of the human race, are intimately connected with the uterus,"

something more closely, we have a notion, (to use a Yankee phrase) than with the parotid gland. Perhaps, however, the author intended by introducing the breasts and uterus into his account of this species, to make the transition easy to the subject of the male organs of generation, for his next division appears under the title of Orchitis. This he commences with an extraordinary fact, to wit, that "man has *commonly two* testicles." In continuation he presents us with Vauquelin's analysis of the semen, and a notice of the last illness and post mortem examination of "Gibbon, the celebrated historian, who, under the classic skies of Lausaune, wrote an immortal work!" Phrenitis, our author considers not only as an exceedingly dangerous, but also as a highly insidious disease. He accordingly recommends prompt and bold measures, though not more so than those long resorted to. They consist merely in the active employment of the lancet, aided by cold applications to the cranium. In illustration of the beneficial influence of the latter, it is related of Mr. Fox, that "when in office during his youthful days, he, after a night spent in dissipation, frequently wrote important papers with a wet napkin round his head."

Pleuritis is considered as being so well understood as to entitle Dr. Dawson to pass over it with "Spartan brevity." We follow his example, and hasten to present our readers with a fine sample of the Doctor's *classic* style and taste, found in the section on Pneumonitis, page 76. Nearly five lines are given on the subject of this species, when the experienced and eloquent author breaks forth in the words following, to wit, "Let me be excused if, strongly urged by private feelings, I digress for a single moment. The great Lord Byron, whose name will forever live, died of this disease after an illness of ten days. He refused to be bled because he had been living most *abstemiously*. His noble and dauntless spirit, which despised the world's applause or censure, fled to Heaven, on the 19th day of April, 1824, at Missolonghi, in Greece. It is pleasing to imagine that his fune-

ral-knell will be answered by echoes which may have smote the ears of Socrates and of Plato. He was the child and champion of Greece—he braved all the perils of Turkish jealousy—he climbed Parnassus—swam the Hellespont—bathed his burning brow in the waters of Helicon—and penned sublime verses on the plains of Marathon. The dear and absent friends of the first of British poets, whose genius was scarcely less versatile than that of Shakspeare, may well exclaim, with Tacitus, 'Mihi præter acerbitatem amici erepti, auget mæstitiam, quod assidere valetudini, favere deficientem, satiari vultu, complexu non contigit." After feasting our readers with the above delightful morceau, we need plead no excuse for passing with "Spartan brevity" over the remaining species of the order under consideration. One word, however, we must say on the 12th. "It is," says the author, "a stranger introduced by myself into the Temple of Nosology." The name, under which this stranger is thus introduced, is Pan-cre-a-ti-tis—his character is undescribed, and the mode by which he is to be managed, left to the wisdom of the priests who serve in the forenamed temple.

Order 3d. Nervous diseases—Sp. 1st. Melancholia. 2d Mania. These diseases our author professes to examine in conjunction, yet on neither is he, in any respect, satisfactory. We can discover, indeed, little else throughout the section appropriated to them, but a mock-heroic encounter of Mr. Lawrence and others, who refer the operations of the mind to physical organization. It is truly amusing to find a writer, in a system of Nosology "constructed for himself, after twenty-eight years of experience," placing mania and melancholia under the head of *nervous* diseases, while, at the same moment, he contends strenuously that they both arise *always* from *moral* causes—are not the result of a disordered state of any part of the *body*, and are never cured by *physical* remedies. The author's views would certainly have been better answered if he had yoked them with Hypochondriasis, (a cousin German of at least one of them,) and given them a place

among functional diseases, the last and most curious of all his orders. But it is not our object to instruct so experienced and learned a practitioner of our art, far as he has fallen short of communicating instruction to us. Mr. Lawrence must feel highly complimented by the following remarks:—"Mr. L." says our author, "asserts, that '*the various forms of insanity, that all affections comprehended under the general term of mental derangement, are only evidences of cerebral affections, in short symptoms, of diseased brain.*' This will not do, and Mr. Lawrence *knows* it will not do." Again, after a series of interrogations and argumenta ad hominem, he concludes that it is not in the manner of Mr. Lawrence, "this material—this Lucretian manner, that the proximate cause of mental derangement is to be revealed—this is not in the spirit of that inductive philosophy which Bacon practised in his life, and illustrated by writings that will never die; no—these are the dregs of the Epicurean philosophy, invigorated by the elegant poetry of Lucretius, and revived and circulated afresh in a new and more imposing form by Cuvier, Lawrence, and other *enlightened* men." But we have violated the Spartan rule of our author under this head, and must make amends for our transgression by passing more rapidly forward hereafter. After leaving several intervening species, without remark, we were stopped in the midst of the section on epilepsy by a passage well worthy of preservation from the oblivion to which we fear the volume before us will be shortly consigned. It is stated that "authors and other gentlemen, on the assertion of Suetonius, affirm Julius Cæsar to have laboured under epilepsy." Believing this to be a gross, unfounded calumny, our author proceeds with the boldness of a true knight-errant to its refutation, declaring his motive to be "the respect he entertains for the memory of so great a man." "I am happy to state," says he, "and the subjoined passage, extracted from a very old and accurate edition of Suetonius, will show, that Cæsar never had epilepsy, but only two fainting fits, 'inter res agendas,' and which

Suetonius, not a medical man, has ignorantly called the morbus commitialis." He proceeds, "no person will be surprised to hear that the 'foremost man of all this world, was used to be terrified during sleep, or twice fainted in a crowded assembly from a *disordered stomach*, who remembers that Cicero mentions, in his oration for king Dejotarus, that Julius Cæsar took an emetic after eating; and when he recollects that supper was the principal meal of the Romans, at which they ate heartily, and fed coarsely, he will not be surprised at Cæsar's uneasy sleep, or the propensity which every Roman had to dreaming." Julius Cæsar, with a disordered stomach—fainting twice in a crowded assembly, after taking a puke, rather than Julius Cæsar epileptic! Did ever Roman dream of such a choice between charges lodged against a Roman emperor?

After this extract, we are sure our readers will confess themselves completely surfeited with the "Nosological Practice of Physic, embracing Physiology," of Dr. Dawson. They will, therefore, suffer us, without a murmur, to leave the remaining orders unnoticed, and we can assure them, that in doing so, we deprive them of nothing worthy of their attention—nothing which they have not hitherto known, even though they may have fallen far short of twenty-eight years of experience. The last order is entirely superfluous, as there is not a species included in it which can justly be considered as being, in any respect, more a disease of *function* than many which are arranged under the preceding orders; there is, indeed, in the whole of this system of "Nosology constructed for the author's own use," and in the mode of treating its details, a puerility which would discredit an inaugural dissertation.

Of the "*classical*" department of this work, as our author terms it, our readers can form some idea from the specimens presented, both as they display his style and his selections. Of the observations termed physiological, they are also able to form some conception. Blumenbach is the author most fre-

quently referred to, we suppose, in consequence of his being fresh in the writer's memory while inditing his work. Yet it is presumed, saith the experienced Dr. Dawson, that "it will not be insinuated he (Blumenbach) taught me physiology, since I have studied it above twenty years, and have only been acquainted with his work five months. If I preserve comparative silence respecting other men, it is not that I am ignorant of their publications, but only that I cannot subscribe to their incongruous hypotheses, illogical doctrines, and inconclusive experiments" Of facts and observations connected with *pathology*, we have found a still greater deficiency than in the other departments comprehended in the promises of the author. The remarks offered on spasm and debility, under the head of fevers, have been already noticed, and, on the same subject, still more may be found interspersed throughout the volume. While speaking of hydrocephalus, he again introduces the latter, as "a bugbear,—the Circe of fevers, which has been more hurtful to suffering humanity than earthquakes to the physical world," and remarks, that "a name, and that name all a lie, has too often been the innocent cause of conducting numbers to the tomb." In the last observation, we agree entirely with the Doctor, though his phraseology does not exactly hit our fancy. We could have wished that he had himself been more completely under the influence of a contempt for *names*, when it first entered into his imagination to construct a nosology for his own use. We should then have been spared the unpleasant task of reviewing labours which we cannot acknowledge have yielded us any thing more than *names*.

Practical facts might have been expected to occupy a considerable portion of the work. Such, however, is not the case. The treatment of almost every form of disease under the respective orders, is comprised in the employment of the lancet, of cathartics, and of opium, with a few unimportant succedanea. On the author's observations on the divine influence of opium, in giving birth to the bright effusions of

the poet, and the commanding eloquence of the orator, as well as his advice to employ it with unreserved freedom in various morbid affections, we should feel it our duty to offer some remarks, did we think it at all probable that the work containing them would be generally read. As we are confident this will not be the case, we need prepare no antidote against the injury likely to arise from counsel so injudicious, and eulogies so extravagant. With this remark we take leave of the author and his offspring. To our *readers* some apology may be due, for having thus intruded upon them the contents and character of a work which we had found to be wholly unworthy of their consideration. But one reason need be given for the course we have pursued. It is a truth, established by every day's experience, that though, as a nation, we are charged with being destitute of taste, though we are ridiculed for catching with avidity, and without discrimination, at every production coming from abroad, there are still individuals found among us who will read, and booksellers, as a consequence, who will import, the veriest trash bearing the title of a work on scientific or other subjects, while the labours of our own countrymen remain unnoticed and unrewarded. To remove this evil, and wipe off this blot from our character as a people, should be the aim of every American journalist, and more especially the object of every American reviewer. And how can this be effected more certainly than by exposing the weakness of those who affect to despise us for our weakness?

Such a cause must lead our readers directly to the conviction that the progeny of dullness can gain nothing by a voyage across the Atlantic; and to the determination by no means to encourage it to leave home. Such have been our views throughout the present article. If we have appeared harsh or ungenerous, let the character of the work be examined before we are condemned for that harshness and want of generosity. We fear no contradiction when we assert, that, at least on this side of the Atlantic, no volume on medicine has appeared for the last twen-

ty-eight years, which more deserved the lash than the "Nosological Practice of Physic, embracing Physiology," by George Pearson Dawson, M. D.

Art. V.—*A New View of the Infection of the Scarlet Fever, Illustrated by Remarks on other Contagious Disorders.* By William Macmichael, M. D. F. R. S. &c. &c. pp. 100. London.

This volume commences with the very interesting remark, that in England the rate of mortality has been greatly diminished within the last forty years, and the author has adduced several very plausible reasons for this circumstance, some of which reflect great honour upon medical science.

There are many who are inclined to scepticism with regard to important truths, and a sufficient proportion of them affect to doubt whether medical art is really beneficial to society, either in augmenting the number or in prolonging the existence of its members. Theoretical argument, however well founded, avails but little against the suppositions of prejudice; nor will a few examples of the obvious success of medical interference, establish its claim to general utility, with those who require evidence that the physician has not in as many instances abridged, as he has prolonged the term of human existence. But, happily, we are not confined to arguments drawn from selected cases, nor to any partial view of the subject, for the purpose of demonstrating the general utility of medicine. In the work before us, its influence upon the rate of mortality of a large nation, during a period sufficiently long to include a fair proportion of all the vicissitudes of health and disease, epidemics and accidents, and disorders indigenous to the country, and we find that within forty years the ravages of disease have been diminished nearly one-third. "In 1780," says our author, "it was calculated that one person in forty (in England) died every year; whereas, ita ppears from the very valuable statistical work* lately published by

* Population and Parish Registry Returns for 1821.

order of the House of Commons, that in 1820 the annual mortality was only one in fifty-eight."

"This increase in the duration of human life is attributable to various causes, among which the most prominent are, the better food and clothing of the poorer classes, the more temperate habits that prevail pretty generally throughout all orders of society, the entire disappearance, or mitigated severity, of many fatal diseases, the substitution of vaccination for the small-pox, and the improved state of the practice of physic.

"It is obvious that whatever effect the last mentioned cause has produced, will be visible chiefly in the better treatment of those diseases which have been called epidemic, and which attack great numbers of persons at one and the same time. It must be confessed, however, that without any apparent satisfactory reason, some maladies have become less common now than formerly, of which rickets and scrofula are the most remarkable; on the other hand, gout, consumption, palsy, and lunacy have increased in fatality. Sudden elevations and depressions of fortune, with the corresponding anxiety and eagerness consequent on an excessive spirit of mercantile speculation, may explain the greater frequency of mental diseases; while sedentary trades and professions; nay, even temporary fashions and customs in dress, must necessarily affect the habits and character of the constitution.

"The indulgences and vices of civilized life naturally account for the greater prevalence of the gout: as to palsy, its increase may probably be fairly enough attributed to the less general use of blood-letting, a practice that was formerly much more commonly employed than it is at present. Twice a year, at what was called the spring and fall, persons at a certain period of life were in the habit of losing blood; and even for the slightest ailment, as a pain in the finger, without any reference to a threatened fit of apoplexy, bleeding was resorted to.

"The obvious causes of all those disorders affecting great numbers of persons at the same time, are bad or deficient diet, noxious exhalations of the earth, contagion, and the general influence of climate, or change of seasons. Any controlling power which we may be able to exercise over these causes will ameliorate the condition of society, and necessarily increase the value of life. Improvements in agriculture, by furnishing a greater abundance of the necessaries of life, counteract the first of these causes. The enforcement of a strict quarantine* has entirely succeeded in banishing one of the

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* "That we can rely on nothing but the strictest quarantine for our exemption from the plague, is proved by the appearance of that calamity in the island of Malta, in the year 1813, when it was imported from Alexandria. The improvements of modern civilized society are, at Malta, in their full force; it is in the possession of one of the most polished nations of the

most formidable of the contagious diseases, viz. the plague; and the *universal* practice of vaccination would as certainly completely exterminate the small-pox. For it has been well observed, with respect to inoculation for the latter disease, that the partial benefit derived by those who undergo the operation, has been overbalanced by its favoring the casual propagation of small-pox, an objection that cannot be made to vaccination

"If it be impossible to get rid of the other contagious diseases, still much good has been effected by disarming them of their violence. Ventilation, the free use of external cold, and the bold administration of purgatives, have contributed much to lessen the severity of typhus, the measels, and scarlet fever."

Our author proceeds to give characteristic sketches of those eruptions which resemble scarlet fever, and which are sometimes confounded with it. The small-pox and measles, he observes, are those which are chiefly apprehended, particularly the latter, because for the former, although the most formidable, we possess a preventative in vaccination, and there is a well founded hope that it will in time become utterly extinct. But we know of no means calculated to fortify the system against the contagion of measles. It is consequently advised, as it is a disease which appears but once in the same person, that children should be exposed to it during a favourable season, after having been prepared by the exhibition of a cathartic. Dr. Home of Edinburgh has attempted to introduce the practice of inoculation with this disease; but the advantages likely to arise from it are not such as will bring it into general use.

"The symptoms of the small-pox," our author observes, "as well as those of the measles, are so obvious and so decided in their external appearance, that little or no doubt can exist as to the fact, whether a person has undergone these diseases or not; but scarlet fever is so various in its character, and there is one form of it so extremely mild, that it does most certainly sometimes pass through the system unobserved, and in many more instances under the name of a *rash*, exciting no alarm, and requiring no medical treatment."

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world; but all these advantages were insufficient to prevent the fatal and wide spreading ravages of the plague, when once introduced into the island."

Dr. Macmichael thinks that the true reason why children are supposed to be more liable to scarlet fever than old people, may be found in the last mentioned fact, namely, that those adults, in many instances, who think that they have never been affected by it, may, nevertheless, have been affected by this disease during infancy, in one of its milder forms, in which it escaped observation.

"There is sometimes in schools what is called a *rash*, a redness of the skin, of various extent and brightness, occasioned by an unusual quantity of blood distributed to that part. Nothing else, perhaps, is taken notice of; the rash passes off without observation, or if it should happen to be for a moment imagined to resemble, in the smallest degree, that alarming disease, scarlet fever, the suspicion is instantly checked, and the whole matter hushed up as quietly and discretely as possible. Now I am anxious to direct the attention of the public to this point; from a judicious practical application of which, I think, much advantage may be derived.

"It has been long indisputably established that the slighest of all eruptive fevers, viz. simple scarlet fever, and that most fatal disease, the malignant sore throat, are varieties only of the same disease, and proceed from the same contagion. That the scarlet fever, like the small-pox, is, in some rare cases, to be had twice, will not be denied, though the *proportion* of such instances which is assigned by Dr. Willan, is, perhaps, unusually small; for he asserts that, in two thousand patients, he had *never seen a repetition* of the disease; that is, that those who have once had the mildest form of scarlatina, have not afterwards suffered the malignant sore throat. It may, however, be safely affirmed, that scarlet fever does not occur twice, more frequently than the other specific contagions, consequently no argument can be founded on such an anomaly."

Dr. Willan is of opinion, that measles, in a very mild form, may occur unattended by fever, and that after the eruption has disappeared, even after the lapse of two years, the febrile symptoms belonging to the disease may be manifested.

Dr. Macmichael, however, does not think that this is true rubeola, but that this mild eruption is nothing more nor less than scarlatina, which not having the power of destroying the susceptibility of the system of the contagion of

measles, this latter disease may, consequently, occur at any period subsequent to the existence of the former.

Our author, however, admits that much difficulty sometimes exists in distinguishing between the eruptions of rubeola and scarlatina; and it is probable that it is owing to their having been confounded with each other that the bills of mortality, during one year, attributed one-thirtieth of the deaths to measles; and, in another, only one in four thousand. It may be objected, he observes, that the identity of scarlatina in its mildest form, is not susceptible of satisfactory demonstration; and that it sometimes affects one member of a family without being communicated to others; and further, that one of a family may have this slight eruptive disease, and others be afflicted, at the same time, by more violent and well-marked scarlatina. This argument, however, does not appear to us to militate, in any important degree, against our author's opinion, that scarlatina does exist in this very mild form. It is, perhaps, impossible to find two constitutions in all respects similar, even in the same family—hence, nothing is more reasonable than that different individuals should be variously affected by the same morbific agent. The matter of contagion may produce a disease more or less violent; or one or another symptom may predominate, so as to hide the identity of the nature of a number of cases from superficial observation.

Our author proceeds to detail the symptoms of this disease in its more aggravated form.

"In the scarlatina anginosa, in addition to the fever and efflorescence, a considerable swelling of the tonsils takes place, beginning sometimes with the febrile accession; at others, not perceptible till the scarlet eruption is at its acme. A stiffness in the muscles of the neck and lower jaw is felt at the very beginning of the attack. On the second day of the fever, the throat feels rough and straightened, the voice becomes hoarse, and deglutition is not performed without pain and difficulty. On the two following days, head-ache, vomiting, delirium, great heat of skin, quick respiration, a

small pulse, extreme languor and faintness come on. Upon examination, a considerable enlargement of the tonsils is discoverable, with a florid redness which extends over the neighbouring parts. The colour of the tongue is deeply red, and the papilæ over the whole of its surface are greatly elongated.

"All these symptoms are much aggravated in the other or most malignant form of the disease. Aphthous ulcers in the throat are visible, sometimes on the very first day of the fever: the sores are numerous, deep, and sordid. The patient becomes hoarse and almost dumb. Sometimes the whole skin is intensely red at the commencement of the distemper; at other times only the breast and arms have this colour. On other occasions, the efflorescence has not been observable before the fourth day of the fever, and in many patients it is scarcely or never perceived at all. The condition of the sores is to be considered as indicating the danger of the disease, which must be judged to be greater in proportion as these ulcers occupy a larger space, are deeper, more firmly fixed, and of a more gangrenous hue. In fatal cases the patient becomes, on the second day, comatose, breathes with great difficulty, bending back his head as far as possible: at the same time, a purulent and highly offensive matter flows from his nostrils; the throat, on inspection, is found to be gangrenous, and death soon follows."

In speaking of the treatment of scarlatina, the author makes the following judicious observation, which is capable of very extensive application in medical practice.

"The two great improvements in modern days, in the management of febrile diseases, consist in the discovery of the powerful influence of diminished temperature, and the beneficial effects of purgative medicines; and in none are the advantages resulting from the use of these remedies more evident, than in their application to the treatment of scarlatina anginosa."

Sponging the body, generally, with cold water, or with vinegar and water, is consequently recommended. But a few years have elapsed since the employment of cold water in febrile diseases, whether externally or internally, was most carefully prohibited, and, notwithstanding the mass of testimony by which it is now recommended, this practice is still looked upon, by many, as at best a dangerous experiment; and there are many among our most intelligent

practitioners who dread the application of cold water, particularly in eruptive diseases. Dr. Macmichael, however, speaks of this practice in the language of one who is not aware that it can be on any account objectionable—he merely advises that it should be employed "only when the skin is hot and dry."

"The dread of the employment of purgative medicines," our author observes, "has now in a great measure subsided; and their cautious use, even in the worst forms of cynanche maligna, has been recommended by some of our most popular medical writers. Formerly it was laid down as a rule of practice in the malignant sore throat, to avoid all purgatives till towards the end of the fever; and if a spontaneous diarrhœa came on, to check it as soon as possible.

It was supposed that purgatives served to diffuse the acrid matter, descending from the throat into the stomach, over the whole surface of the intestines, and thus increased the source of contagion, and aggravated the irritation which arose from the presence of this deleterious matter. But the moderate and free evacuation of the bowels, by a few grains of calomel, combined with rhubarb or jalap, so far from causing the mischievous effects we enumerated, will expel the acrimonious and offensive mass, and instead of increasing the debility, will relieve the symptoms of general fever, and, united with the cooling treatment above insisted upon, most materially expedite the cure of the disease.

Acidulated drinks are extremely grateful; a few drops of *oxygenated muriatic acid* diluted *in* water form, perhaps, as agreeable a refrigerant as can be contrived."

We perfectly agree with our author that acidulous drinks may be both agreeable and salutary, in the disease under consideration; and although we possess a great variety of substances from which these drinks can be composed, we must, nevertheless, regret that we have never met with the particular acid which Dr. M. prefers and has named above. There is a gaseous substance which once bore the name of oxygenated muriatic acid, but as it does not possess any property of an acid, Dr. M. cannot allude to it; at least, we presume, not from his experience in its use.

Epispastics are also recommended, particularly with a view to diminish the quantity of acrid mucus secreted by the inflam-

ed tonsils; and gargles, also, for the purpose of washing away this offensive matter, which otherwise would pass into the stomach and probably give rise to distressing diarrhœa. Ultimately, when all the febrile symptoms have disappeared, our author recommends the exhibition of cinchona to confirm the health and restore the strength of the patient.

We regard the present work as one well calculated to excite our attention to the distinguishing marks of those diseases, many of whose more prominent symptoms are so analogous as to occasion their indiscriminate diagnosis. Dr. Macmichael has, in the course of his "view, &c." given us good reason to believe that scarlatina, rubeola, urticaria, and occasional cutaneous eruptions arising from gastric irritation, have been confounded with each other, much to the prejudice of correct pathological description. T. M. H.

ART. VI.—*A Treatise on Syphilis; exhibiting the advantages of large doses of the sub-muriate of mercury in the cure of that disease: also an inquiry into the modus operandi of mercury. Illustrated by experiments.* By JAMES BOYLE, Esq , one of the Surgeons of the United Institution of London and Westminster, for the treatment of lying-in women, and children, &c. &c. London, 1824. 8vo. pp. 166.

IT has of late been quite fashionable to decry the use of mercury in venereal affections. Its baneful consequences have been set forth in all quarters—in books and in occasional essays—in the lectures of professors, and in conversation—every where the alarm has been sounded, concerning the pernicious influence of this metal. But the tide now begins to roll back—and mercury is again looking up to take its former station in the list of anti-venereal remedies. Whatever may have been urged against the employment of this article in venereal diseases, we feel persuaded that it will long, and perhaps ever, remain the most important and effectual remedy in affections of this kind. It must be acknowledged, however, that though a powerful and a safe remedy, when properly

managed, its abuse or improper administration is calculated to do much mischief;—and this, indeed, can be said of every important article of the materia medica. If we are not greatly deceived by what we have ourselves seen, we would say that the author of the book before us, has recommended this remedy in a way, which, in the majority of cases, is not only unnecessarily liberal, but also calculated to do much injury.

The author commences by objecting to Hunter's doctrine concerning the incompatibility of two diseases in the system at one and the same time. The reverse of Hunter's opinion is indeed now pretty generally acknowledged by medical writers. The author has seen the epidemic cholera of India attack patients whilst under the influence of mercury, for the cure of chronic dysentery; and where, consequently, these distinct diseases existed in the same person simultaneously. He is averse to the opinion entertained by some that gonorrhœa and chancre are diseases specifically the same.

"It is exceedingly probable," he says, "that when chancre appears to produce gonorrhœa, and gonorrhœa chancre, the matter in either case is mixed. It is no uncommon thing for a woman to give gonorrhœa who never felt a symptom of the complaint herself. Under such circumstances, we believe the poison had remained in a quiescent state in the vagina. It is perfectly possible, then, for a woman in this state, having a chancre or chancres in addition, to communicate one or both the diseases."

Dr. Boyle thinks that in all cases of true venereal chancre, the disease is constitutional before it appears as a local affection: "that is to say, that absorption of the poison had actually taken place, previously to the ulcerative process: and that such a change in otherwise healthy parts, is a proof of the constitutional affection, and not a cause of it." He is also of opinion that no bubo can be considered venereal unless it be attended by a sore. The arguments by which he endeavours to sustain these positions, are however far from being satisfactory. If the diseased action were not constitutional before it appears locally, says he, "we should have

chancres making their appearance almost immediately after the act of coition with an unclean person, and not at the expiration of days and weeks as is generally the case." But if the disease is always constitutional before it appears locally, how are we to account for the fact, that a purely local treatment of the chancre, on its first appearance, will generally put a stop to the progress of the disease? Certainly, were it *ab initio* constitutional, the destruction of the local affection, however early practiced, could not eradicate the constitutional affection. It is unreasonable to expect to remove a disease merely by removing one of its effects.

He states that he has never seen a bubo that came on without a sore at the time, or soon after, which required mercury for its removal. We have seen some cases from virulent gonorrhœa which resisted every attempt at removing them, until mercury was administered; and we have no doubt that the majority of physicians who have had sufficient opportunities of prescribing in this disease, have met with similar cases. It must nevertheless be observed that the removal of a disease by mercury, after it has resisted other means, is no proof of its being venereal.

The author next enters upon an examination of what is now called the new practice, that is, the non-mercurial treatment of this disease. He shows that there is nothing new in this mode of managing the complaint, and that it was well known and practised more than three hundred years ago. He "is quite satisfied that in the East Indies, the natives cure the disease without the use of mercury. They are in the habit of giving some sudorific vegetable composition, during the administration of which the patient is confined to his house or hut, for the purpose of warmth." Still he contends, that the employment of this remedy is decidedly the most certain, safe, and expeditious mode of treating the malady.

"I think," says he, "the writers in favour of the new practice, as it is called, have themselves given evidence whereby to condemn it; for admit-

ting the point of contention, that the disease may be cured without mercury, what man, in his *sober senses*, would feel disposed to suffer a chancre upon him from one to three months, with the additional prospect of being soon after a still longer time under medical discipline, for what usually follows, ulcerated sore-throat, copper-coloured spots on the skin, nodes, diseased bones, &c. &c.; when, as I propose to prove, the complaint, in the first instance, may be radically cured in a few days!"

Bubo.—Bubo, or swellings in the groin, may arise from various causes, and it is of much importance to attend to this circumstance in prescribing for their removal. It may arise from cold; from any violent exertion, such as jumping, dancing; from the irritation of a *corn*, and from cutting too closely one of the toe nails.

The author says, "I had a patient some years ago who presented himself with a sore on the *penis* and two buboes. He declared most solemnly that he had not had connexion for nearly two years, and that it was not possible this could be venereal: he was a sailor, and therefore did not say so from delicacy. I confided in his statement, pointing out to him the impropriety and danger of deceiving me: I dressed the sore simply, and endeavoured to discuss the tumours. What appeared to be a chancre healed in a few days; but the swellings in the groin suppurated, in spite of all my efforts to the contrary. It was nearly seven weeks before this patient was cured. I saw him about twelve months after, and there had not been the slightest appearance of secondary symptoms. Here, had I been less credulous, this poor man would have been doomed to a tedious course of mercury, to say nothing of the consequences attending it!"

The author does not think it necessary, in the treatment of chancre, accompanied by bubo, to continue the mercury till the disappearance of the tumour. In this opinion we perfectly agree with him, and think the observation worthy of particular attention. Indolent buboes, he thinks, are best treated by hot applications. "Experience," he says, "has taught us that warmth does not always favour the process of suppuration. On the contrary, it is well established, and I believe the idea originated with Mr Brodie, that heat will invariably direct its influence to the course upon which nature has determined; and that it will expedite that process, whether it be resolution or suppuration." In stationary tumours of this

kind, camphorated mercurial ointment, or camphorated spirits, applied *with a bandage moderately tight*, is perhaps the best application we know of.

Sloughing or Phagedenie Chancre.—The author does not describe the appearances and character of this species of venereal ulceration. He confines his observations altogether to its remediate treatment.

"The whole of the local disease," he says, "consists in a diminution of vital action, or vital energy, in the absorbents and minute blood vessels." Hence, almost all practitioners agree that stimulating applications, such as hot fomentations or poultices, camphorated spirits, alcohol, &c. &c. are the most useful. Internal stimuli, also, are generally considered as indispensible. The author recommends in strong terms large doses of mercury. "I have seen large doses of the sub-muriate of mercury," says he, "cause a line of separation between the sound and unsound parts, when various lauded remedies proved to be entirely useless."

"In a venereal sloughing ulcer," says the author, "we have two objects in view; one, to stop the progress of a local affection; the other, to eradicate a systematic or constitutional disease, of which the former is symptomatic only. If both these circumstances can be effected at the same time, no one surely will object to the propriety of the measure: this, on the contrary, should be the grand object; and analogy, I think, bears me out in a confident opinion, that large doses of the sub-muriate of mercury are best calculated to effect those important and especial purposes."

Large doses of mercury are, according to our author, much less apt to do injury in scrophulous habits than small ones.—This he predicates not only on the result of his own experience, but also on the alleged fact, that this medicine acts most as an irritant when the system is brought under its influence by means of small than by large doses.

Modus Operandi of Mercury.—The author objects to Hunter's opinion that the remedial powers of mercury in this disease are in any way to be explained on the principle of evacuation. Hunter adopted this opinion from observing that eva-

cuations generally do not effect a cure. "This," says our author, "is a hasty and inapplicable conclusion."

"The complete revolution," he continues, "which takes place in the system during the state of ptyalism, by an increased action in all the secretory and excretory organs consequent to this state, are of a very different nature from that kind of evacuation which is the mere result of a simple purgative; and which, by way of distinction from all others, might properly enough be termed the *specific mercurial evacuation.* It is not till after the salivary glands appear affected by mercury, that any rapidly favourable change takes place in chancres. During the first few days of administering the medicine, it is almost invariable to observe slight febrile symptoms, and a greater degree of irritability in the sores. But when the salivary glands are affected, the last and most important functional change which the mercury is capable of inducing, we find the general irritation greatly lessened, and the sores then heal rapidly."

The fact that many cures are effected with this remedy without the salivary glands being in the slightest degree excited, is not to be received as a proof that this article does not produce its remediate effects by exciting evacuations. It is true, says the author, that we often see morbid states of the system changed by a few alterative doses without affecting the mouth. "In such cases, if minute attention be paid, an increased action may be witnessed; whether proceeding from the emunctories of the skin, from the liver, the kidneys, the intestines or the stomach; and as this effects purposes not within the powers belonging to common diaphoretics, dioretics, purgatives, or stimulants of any kind, it must be attributed to a specific mercurial evacuation, however that evacuation be effected.

The author, therefore, believes that the modus operandi of mercury, in the cure of diseases, is by exciting a *specific evacuation.*

"The assertion," says he, "is demonstrable that mercury acts in the cure of the venereal as it does in the cure of every other disease, namely, by keeping up an increased action of the glands, whereby a more than ordinary secretion is carried on, till the system becomes freed from poisonous, extraneous, or irritating matter, whatever be its nature, or however it be induced."

His arguments, however, are by no means sufficient to sustain this opinion. That the emunctories generally are excited by this remedy, is unquestionable, but that the cure is mainly or in any degree dependent on the evacuation thus produced, is exceedingly doubtful. The new action throughout the whole capillary system which this remedy produces, and upon which the augmented secretions depend as an effect, is in itself sufficient to account for its remedial impressions. If mercury cures a disease more rapidly when pushed to the extent of producing salivation, it is not because a greater evacuation is effected, but because the specific excitement, throughout the system generally, is more intense, and consequently more apt to overcome the diseased action, for the removal of which the remedy is administered.

The following observations, favourable to our author's sentiments on this head, deserve attention in a practical point of view:

"It frequently happens," he says, "in *advanced* stages of this disease, that mercury is administered as our only hope; and affecting the system by means of it, is the object in view; when, if ptyalism, or a flow of saliva be excited, the patient almost invariably recovers; but if, on the contrary, a *sore mouth*, that is to say, ulcerated gums, &c. take place, and that it is impossible to establish ptyalism by perseverance in increased quantities of mercury, there is, I believe, no hope of the patients' recovery."

Under the head of *anomalous effects of mercury*, the author makes the following observation, which we have seen verified in several striking instances:

"It sometimes happens, that a very violent salivation breaks out suddenly in cases which had long previously resisted mercurial action: this circumstance, however, can generally be traced to some evident cause; such as after exposure to the open air, free living, and much exercise, the patient, whether from necessity or choice, changing his habits for a more steady life, and confining himself to bed, or an apartment in which the air was close. Of the truth of this the following are striking illustrations:

"A gentleman, some years ago, was under my care in the Mediterranean, for the cure of a venereal affection. He was a free liver, and took much exercise on shore; and although he had been taking mercury for fourteen weeks, yet its action upon the salivary glands and upon the disease, was per-

ceptible only. At length, being obliged, from debility, to confine himself to bed, a violent and sudden ptyalism took place, and the venereal symptoms immediately disappeared.

"Subsequently to this, in the East Indies, a gentleman came under my care for the cure of a severe hepatic affection; his former medical attendant had given him mercury to a great extent without being able to affect his mouth, or bring about any change in his complaint. I confined him to bed, when almost immediately a copious ptyalism was the result, and his disease as quickly ceased to be an inconvenience."

The author thinks that the symptoms which Pearson points out as presaging a fatal result, will never arise from this remedy, if the system be suddenly put under its influence by large doses. "It is only from reiterated action," says the author, "of the nervous upon the vascular, and the vascular upon the nervous systems, and that by long continued small irritating doses, that mercury acts as a poison, gradually unhinging all the important organs, till, at length, the vital spark is annihilated."

The author observes that large doses of calomel never cause violent purging.—It is certain, that the cathartic effects of this medicine are seldom proportionate to the size of the dose, and that five or six grains will in general produce as much catharsis as twenty or thirty. "The purgative effects of this medicine," says he, "appear to decrease in a given ratio after eight or ten grains." He further states, that violent ptyalism "is infinitely less common from large doses of calomel than from a protracted course of mercury, in whatever form exhibited." This may be the case in some instances, but, as a general observation, it is by no means correct. Some of the most violent cases of ptyalism which we have seen, and we have seen some uncommonly violent, were produced by large and repeated doses of calomel. The fact, however, appears to be, that the degree of ptyalism depends more on the degree of susceptibility of the system to the action of this remedy, than on the quantity introduced into the system.

Where ptyalism is excessive, the pure acetate of lead, applied by means of a hair pencil, is said to be very serviceable. Tar has also been recommended as an efficacious application to ulcers of the mouth from mercury.

Experiments illustrative of the operation of Mercury.—The author details six experiments under this head, which give us no other information than that dogs may be killed by stuffing them liberally with calomel! This "important result," as he calls it, convinces him that mercury does not kill in the manner of a poison, but simply by producing a total indisposition to take food,—that is by inanition!

Preparatory measures to the administration of Mercury.—Blood-letting, where the pulse is full and active, is an important preparatory measure to the exhibition of this remedy. The author explains the modus operandi of blood-letting, in this respect, as follows:

"When an increased action in the blood-vessels and consequent distention takes place, diminution of diameter of the absorbents, or obstruction from pressure of the surrounding vessels, if not diminution of diameter, will be the consequence in certain parts of those vessels. And the fact of blood-letting under circumstances of plethora and febrile irritation so quickly influencing the operation of mercury, whose powers had been previously resisted, stands forward, I think, as an incontrovertible testimony of the reason of the causes to which I have assigned this particular result. As might be anticipated, *a priori*, the distended vessels being unloaded by the operation of venesection, full play is permitted to the absorbents: the veins, too, which from the late discoveries of Magendie, we know to possess an absorbent power, are, when thus relaxed, enabled to perform their additional function."

The absurdity of this explanation is so evident, that it scarcely needs any notice in this place. If blood-letting, as a promoter of the action of mercury, acted simply by removing mechanical pressure from the absorbents and veins, then we should find debilitated and nearly exhausted persons exceedingly susceptible of the specific influence of this medicine. That this is not the case, however, every practitioner must be convinced from his own experience.

The mode of administering Mercury.—The author, as we have already stated, advocates the propriety of administering this remedy in *large* and frequent doses, so as to put the system under its influence as speedily as practicable. "Nothing," says he, "can be more glaringly inconsistent than the practice of administering small and ineffectual doses of mercury in the cure of venereal, allowing the constitution to become habituated to its use, and consequently insensible to the action of the specific, whilst the disease and debility are extending with destructive strides."

"If, for example," he continues, "two patients, under precisely the same circumstances, entered on means of cure at the same time; the one shall be put under a five weeks' course of mercury, so conducted as to make the mouth sensibly affected at the end of that time, at which time also, his sores or other venereal symptoms shall have disappeared. The other patients' mouth shall be affected in two days, his sores heal in four or five after, and the salivary glands continue in a state of excitation for six or eight days more. I ask, which of these patients is likely to suffer most from the effects of the disease and the remedy? Assuredly, if we admit the venereal disease to be capable of acting upon the constitution generally, we can have no hesitation in saying, the sooner that power is destroyed the less injurious will be its effects on the constitution; and the reverse of this on procrastination."

The author then adds a number of cases illustrative of the utility of administering large doses of calomel. We extract a few of these cases:

"A gentleman consulted me on account of a sore, situated behind the corona glandis, on the right side of the penis, and extending to the frenum, which, by a destructive process, it finally destroyed. This sore was the almost immediate consequence of an imprudent proceeding, and had been increasing in size and malignity of appearance till the time of my seeing it; a space, to the best of the gentleman's recollection, of nineteen days. There was every characteristic mark of true venereal chancre about this sore, and which, added to the circumstance of the subject of his amour, left but little doubt as to the nature of the disease. An attempt, however, was made to heal the sore by dressings, quiet, and other means likely to effect this under ordinary circumstances, but in vain, without

mercury. It was unusually difficult to affect this gentleman's system, *eight scruple doses* of calomel being necessary to induce ptyalism, and effect a cure The sore, in consequence of a scorbutic diathesis in the system, underwent many changes, requiring the aid of various local applications, and the occasional exhibition of an opiate: he was eighteen days under cure. A short time back, this gentleman had occasion to put himself under the care of a practitioner in London, for the cure of a recent infection. On this occasion, he was treated by the common mode of administering mercury, and his sores were about seven weeks in healing, although his general health was then good.

"James Sparks, seaman, entered the sick-list for the cure of a chancre—it had commenced about fifteen days previously. January 30th, prescribed calomel, gr. xxv., to be taken in the evening. 31st, two copious alvine evacuations; gums painful, and evidently swelled. Repeat the dose. Feb. 1st, spits freely; no alteration in the sore. 2d, mouth more affected; sore begins to heal at its edges. 3d, still spits copiously; sore healing rapidly; has had no alvine discharges. Ordered sulph. magnes. 4th, bowels open; sore almost healed. 5th, sore healed. Tenderness of the mouth continued till the 13th.

"Samuel Filley, with venereal chancre on the *corona glandis,* took, on the 24th August, ℈j of calomel—this produced three evacuations from the bowels. 25th, mouth sore; 27th, ulcer much cleaner, and commencing to heal; gums much affected. Sept. 2d, calomel, gr. xx., with opium gr. iss. 7th, sore healed."

In a letter, addressed by Dr. Reid to the author, there are several cases mentioned which afford striking evidence of the advantages to be obtained, in some violent cases, from a prompt and vigorous mercurial treatment. Dr. Reid mentions a case that occurred to him in the Mediterranean, which he at first attempted to cure without mercury; "for some time things went on very well; the sore was nearly healed, when all at once, at least within twelve hours, the sore assumed a sloughy appearance, and, in twenty-four hours more, the most malignant ulcer, emphatically called *black lion*, became fully established, without any apparent deterioration of the patients' general health." Various powerful means were employed to arrest the progress of the ulceration, but without any advantage. Dr. Reid finally administered thirty grains of calomel, and had mercurial ointment rubbed into both

thighs.—"In the evening, the ulcer looked truly frightful:" —two scruples of calomel were given, and mercurial frictions continued; the ulcer was dressed with calomel and a carrot poultice. Next morning, the sore looked better—brassy taste in the mouth—bowels but once moved since he took the calomel. The ulceration was arrested, and healed in a reasonable time. "In this case," says Dr. R., "I remember very distinctly, that so far from the large doses of calomel exciting violent purging, they only produced one motion; their action was decidedly and directly sedative; and it afterwards required brisk purgatives to restore the intestines to their healthy action." Dr. R. mentions another case of a similar character, in which he was obliged to give five *scruple doses of calomel* in twenty-seven hours, before the phagedenic ulceration could be checked. We have ourselves seen phagedenic chancres arrested in their destructive progress by large and repeated doses of calomel. We have given three scruple doses in twelve hours, and in twenty-four the good effects on the disease were manifest. Even in ordinary venereal chancres we have lately administered at once, in the beginning, several large doses of this medicine, and have had the satisfaction to see them heal speedily.—We would not, however, advocate this practice in the usual cases of this disease.—These may in general be readily cured by local escarotic applications, in their commencement, and an alterative mercurial course in their more advanced stages, without subjecting the patient to the disagreeable effects of salivation.

ART. VII.—*A Treatise on Stricture of the Urethra; designed as a Manual for the treatment of that complaint; and addressed chiefly to students and junior practitioners.* By GEORGE MACILWAIN, Member of the Royal College of Surgeons, &c. &c. Octavo, pp. 138. London, 1824.

THIS is an excellent little work, and contains more good practical matter than can usually be selected from a much larger number of pages. In our analysis of it we shall, therefore, be enabled to present to our readers some useful and interesting information, although, perhaps, nothing of a very novel character can be pointed out in the whole treatise.

The most frequent situation of stricture is, according to our author, in that part of the membranous portion of the urethra which is immediately posterior to the bulb; the next is immediately anterior to the same part. From this point forward every part is liable to contraction, but not equally so. The external orifice is but seldom affected with stricture, while that portion of the urethra which is the seat of gonorrhœa is very often contracted—indeed, according to our experience, more frequently than any other part of the whole canal. Our author thinks that the cavity of the bulb is never constricted, and he asserts that the prostatic portion of the urethra cannot be diminished by this disease. He might have observed, however, that an enlargement of one or other of the lobes of the prostate, is capable of so far compressing the parietes of the canal together, as both to impede the flow of urine and obstruct the entrance of a catheter.

The different varieties in the form and extent of strictures are thus accurately described by the author:

"There may be a simple narrowing of a part of the canal, as if a ligature had been thrown around it: the opening by which its different portions communicate being in the centre, or the contraction being irregular, the opening may be placed superiorly, inferiorly, or laterally. In general, the narrowing is limited to a line or two in length; but in some cases it extends much farther, the canal having a continuous diminution in its calibre for an inch or more: and again, there are instances in which the whole tube, from

its membranous portion forwards, has become lessened in its diameter. Sometimes there is a sort of bridle thrown across the urethra; and the impression, in a case of this kind, which is made on a soft bougie, resembles that which would be produced by drawing a piece of fine twine across its point. I have met with several cases of this kind; and if it be not frequently observed in dissection, I should be disposed to account for it by the appearance being, in all probability, destroyed in slitting open the urethra. It does not appear, however, to have escaped the observation of the accurate Morgagni, who speaks* of having found 'fibres and fleshy fibrillæ in the urethra.'

"The change which the mucous membrane undergoes, consists of a greater or less thickening. In some instances it proceeds so far, that no similarity with its healthy condition is discoverable, appearing almost of a cartilaginous texture, whilst in others scarcely any change can be observed; and there are degrees of alteration intermediate between these extremes. Where the contraction is considerable, and the membrane but little altered, there is a thickening produced by deposition exterior to it. The change of structure is generally proportionate with the duration of the complaint; and if this has been very long, the whole of the urinary organs usually become more or less diseased."

It is generally supposed that all strictures result either from some form of gonorrhœa, or from the treatment which is pursued in the cure of that disease. The following observations, however, will serve to show the existence of other causes which occasionally, although by no means so frequently, as our author supposes, do contribute to the production of stricture.

"Peculiarity of constitution predisposes some individuals to stricture; although it is but seldom that we can trace it as unequivocally resulting from this idiosyncrasy, independent of the intervention of local excitement, for reasons which obviously present themselves on considering the functions of the generative organs. As no part of the body is exempt from the influence of general constitutional disorder, so may the urethra become the seat of its local development. In the present state of science it is not necessary to enforce the truth of this proposition by any laboured reasoning. The effects frequently produced by stimulating or other substances taken into the stomach, on the urethra, in persons who have stricture, as well as in those who have not, are objects of familiar observation. The least deviation from their ordinary mode of living will, in some individuals, induce

* Vide Lib. xlii. Sec. 41, et seq.

irritation in the urethra or bladder, where there is no evidence of disease in these organs. Further, all cases of stricture are invariably relieved by a moderate and rational diet, and the symptoms uniformly aggravated by a contrary line of conduct. A volume might be filled with facts and arguments leading to the same conclusion; but at this day it would be a work of supererogation. Habitual abuse of wine or ardent spirits will induce stricture; but, whether by an influence directed to the health in general, or to the urinary organs in particular, it is difficult to determine. There is no cause to which strictures are so frequently referred as gonorrhœa; but whether it exerts so extensive an influence in their production as is generally supposed, may be fairly questioned. Stricture often takes place at a period very remote from the existence of any affection of this kind, and in patients who never had gonorrhœa in their lives, as also at an age which must be altogether exempt from the agency of such a cause. I shall not pursue this subject further, as Mr. Hunter's book contains facts and observations which any candid man must consider as conclusively establishing the fact, that gonorrhœa is only to be considered in common with other causes, though perhaps more frequent than many others. Injections no doubt give rise to stricture; but I cannot agree with Sir Everard Home to relinquish their employment, because the ill consequences which have sometimes followed it I believe to be referable to their having been injudiciously administered. The experience of Mr. Hunter showed that strictures occurred as frequently after gonorrhœa treated without injections as with them; I could readily believe much more frequently, and for the following reason:

"Diligent investigation shows that stricture does not commonly proceed from violent inflammation, but from a sort of irritation, moderate but continued, principally characterized by a morbid increase of sensibility, sometimes attended by discharge. Thus it is found that where stricture can be traced to gonorrhœa, the latter has been neglected, and the irritation, of which the discharge is sufficient evidence, unnecessarily protracted; during which period the patient is liable to sources of excitement which he is wearied with foregoing. No state can be more favourable to the formation of stricture than that to which I have just referred. Injections, therefore, when properly employed, by removing the irritation and discharge, so far from conducing to the occurrence of stricture, become actually indirect preventives of this disease. The consideration of gonorrhœa would lead to a digression; I shall therefore merely mention the principles to which it is necessary to attend in the employment of injections. First, they should never be used whilst there is pain in micturition; 2dly, their quality should never be of such a nature as to produce more than a temporary smarting; 3dly, their strength should be increased as the canal becomes less susceptible; and, lastly, their action should be confined to the seat of the disease. Immoderate indulgence in sexual intercourse is with propriety considered

as a cause of stricture. It is obvious that the continued irritation and excitement to which the organs are thereby subjected, must be very favourable to its production. Calculus in the bladder, enlargements of the prostate, and affections of the rectum, are occasional causes of stricture."

Under the head of "*irritable urethra*," our author has introduced some important observations. According to him, all strictures are preceded by an irritable state of the canal, the detection of which will lead to the prevention of their occurrence.

"The urethra, however, may labour for a considerable period under this affection before contraction takes place in any part of it, of which I have seen repeated examples. The morbid sensibility which is its principal feature may affect the whole canal, or be confined to one portion of it; in which latter case it is almost invariably situated in that part enveloped by the prostate gland. As the treatment usually includes the occasional introduction of instruments, it may at first appear unnecessary to attempt any other diagnosis than these are capable of affording. But when we consider, first, the great and in many cases the insuperable objection made by patients to the introduction of instruments; 2dly, the difficulty of making them understand how their introduction can be necessary where there is no stricture; and, 3dly, that some cases admit of relief without their employment—the diagnosis becomes important: and although it is confessedly obscure, yet I believe, in many cases, it is to a discriminating practitioner sufficient to enable him to distinguish the two maladies. First, then, with reference to the volume of the stream in micturition. Persons with irritable urethra will occasionally make an exceedingly small stream, yet at other times it shall be of the natural size: now, although in stricture its volume varies considerably at different periods, yet it is never to the same extent; attentive inquiry showing it always to be less than natural. Again; we find also that the diminution has progressively become more considerable, which is not the case in simply irritable urethra. It is by no means uncommon to find patients making water in a full stream after a meal, although previous to this it was exceedingly small, and the urethra so irritable as to give a feeling of strangury—a difference which never happens to the same extent in stricture. Involuntary emissions* are occasionally attendant on stricture,

* I have seen very many cases tending to establish this point. This morning I had occasion to examine the urethra of a patient who consulted me a few days since for symptoms which rendered him confident that he had stricture. On his informing me, that since he had employed the mea-

but they are scarcely to be considered as a common symptom; whereas in simply irritable urethra nothing can be more frequent: and I am the more confident that this difference exists, from finding that they are sometimes described as a symptom of stricture occurring at an early period of the disease.* Where these are a leading symptom, the irritation is frequently confined to the prostatic portion, but not invariably, as I have seen examples of the contrary. As far as my observation goes, a greater or less disorder of the general health usually accompanies this affection, whereas in stricture it is frequently by no means so easily discoverable; it would seem as if this farther development of disease had rendered the character of it more local, except indeed where the symptoms have made such progress as to react on the general health in their turn. Irritable urethra, besides giving rise to symptoms somewhat analogous to those of ordinary stricture, will sometimes induce others, which may be considered as the occasional attendants on that complaint. The former we need not mention at present. By the latter, I mean abscess in perinæo, painful enlargement of the testicle, and hydrocele."

The treatment of irritable urethra laid down by our author is so rational, that we need make no other comment upon it, than to observe, that we have been in the habit of pursuing almost exactly the same mode for some years past, with the most decisive advantage and success.

"In the treatment of irritable urethra it should be observed, that there are some cases in which change of air and attention to the general health are alone necessary, and others in which nothing else will relieve the symptoms. In general, however, the treatment consists in the introduction of instruments directed by a principle not altogether previously unknown in surgery, but first applied to the urethra, as I believe, by Mr. Abernethy, to whom the Public and the Profession are otherwise so much indebted. This consists in diminishing the morbid sensibility of the canal, by habituating it to the mechanical stimulus of an instrument, on the same principle as we do ulcers to the chemical influence of our common stimulating applications. We should commence by an instrument calculated to produce the

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sures recommended to subdue the irritation, his stream had been on some occasions as large as natural, I recollected that he had not complained of involuntary emissions. On making an inquiry as to this point, he said that he was very frequently troubled with their occurrence. Examination detected a highly sensitive prostatic urethra, but no stricture.

* Howship on the Urinary Organs.

least irritation, and increase this in proportion to the diminishing susceptibility of the urethra. No instrument should be introduced until those measures have been employed which should precede the introduction of instruments generally, for which I refer the reader to the treatment of stricture. When we have employed those for a few days or a week, a well-made highly polished elastic gum instrument, enclosing a fine wire, should be introduced, and repeated at intervals, longer or shorter in proportion to the improving state of the urethra. In general, once a-week will be sufficiently often, except towards the conclusion, when it may be done twice during that period with advantage; and as metallic instruments generally produce more irritation than those of elastic gum, they may at the same time be substituted for those of the latter description. As a small instrument is difficult to introduce, produces more irritation, and gives us no information of the real condition of the canal, should there be stricture, their use is to be avoided; yet it is not advisable in this case to employ one of the full size, because it will produce too much irritation for our present purpose, the canal not being capable in this condition of submitting to the natural distention of its sides, which, if it be forced, will be productive of an increase rather than a diminution of irritation. I generally commence with No. 10, increasing its size at every subsequent introduction. In some cases (notwithstanding the employment of measures recommended previously) we cannot introduce an instrument on the first trial, by reason of the excessive irritation it produces; but we always succeed on the second or third attempt, for although the bougie may have only passed through a portion of the canal, its presence there will in a degree relieve the irritability of the whole. On the first and second visit, the instrument should only be allowed to remain in the canal for a few seconds; its residence there being gradually prolonged, so that at last it may be allowed to remain a few minutes with advantage. Rest will very much facilitate the favourable progress of these cases. I have, in general, however, had patients whose avocations denied them this advantage; I never, however, neglect enjoining it where circumstances admit of their compliance. If motion should be found prohibitory of improvement, the bougie may be introduced the last thing at night, when the individual will often be enabled to resume his ordinary avocations in the morning with impunity. Throughout the whole treatment, those measures which are either directly or indirectly calculated to relieve irritation in the parts concerned, should be rigorously enforced, and which will be mentioned in connexion with the treatment of stricture."

On the subject of "the introduction of instruments in general," Mr. Macilwain has displayed a great deal of practical knowledge. He prefers the cylindrical form of the bougie "since if it be conical and its passage be obstructed, we are

at a loss to know if the obstacle impede the point, or whether this may not have passed a stricture which is not capable of admitting its increasing diameter." We hardly consider this, however, as a sufficient reason for the preference he thus so decidedly expresses—inasmuch as we have often succeeded in introducing a conically pointed bougie after having been baffled in attempting to introduce a cylindrical one. Nor are we disposed to agree with him in the opinion that "an elastic gum bougie should never be employed without a wire in its centre," for the presence of a stilet within a catheter has often appeared, in our hands, to impede the passage of the instrument through the urethra, instead of facilitating it. In metallic instruments it is desirable that the curve should correspond to that of the urethra. Our author thinks that the curves recommended by Sabatier and Dessault are much too large: he prefers the form adopted by Mr. Stanley, which is altogether straight, except within about two inches of its point, where it is gently, or gradually bent. The largest instrument which the size of the stricture will admit, is generally introduced with the greatest ease. He believes that practitioners have often contended with imaginary strictures from using small instruments. One gentleman had often been sounded with small bougies, which could never be made to pass far up the urethra, or to relieve the symptoms of stricture. Our author, however, immediately introduced a full sized bougie on the first trial, and ascertained that a mere irritability of the urethra had produced all the symptoms of stricture.

He does not approve of the use of dilators of any kind for the cure of strictures, and the following extract will no doubt serve to justify his opinion in the minds of our readers:

"It is not consonant with reason to suppose, that we can stretch living parts as we can inanimate substances, possessing an elastic property; yet we must think after this manner if we have recourse to the dilators; although, as before observed, I have not been in the habit of using them, I have frequently seen the disadvantage of increasing the size of the instru-

ment too rapidly. They never can be of any use except where the stricture is capable of admitting an instrument, however small; and, in such a case, the restoration of the canal can be effected by other means which enable us to regulate the progress of the dilatation with mathematical accuracy. I would beg also to observe, that doubts may justly be entertained whether, by the latter plan, the stricture is removed by dilatation, strictly so called. Many canals of the body certainly, in their healthy condition, admit of this dilatation, and so may the urethra when strictured, provided the alteration in structure be inconsiderable, since the natural texture of the lining membrane does not seem prohibitory of it. But where those changes have occurred, which uniformly attend cases of any considerable duration and severity, it is more probable that the pressure of the instrument produces absorption, either ulcerative or otherwise, of the newly formed substance; I am, however, of opinion that the dilator is an instrument always unnecessary for the removal of stricture, and frequently improper. For the foregoing reasons a bougie or silver catheter is to be preferred, where it is the object to remove the contraction by dilatation; to which may be added another, viz. the facility with which, in most cases, this is effected.

"Some persons have, in severe cases where no instrument could be passed into the stricture, endeavoured to overcome the obstruction by thrusting a conical catheter, or other firm instrument, onwards; I dare not say through the stricture, for the probability is that it rarely takes the route intended by the surgeon. Indeed, this practice is fraught with so much danger as never to be admissible. There are many facts, showing the difficulty and mischief attending it, which should absolutely prohibit any prudent surgeon from venturing on its employment. Firstly, the most accurate knowledge of the anatomy of the parts will be insufficient to enable us to ascertain that we are employing force in the right direction. Secondly, it should be recollected, that a strictured portion of the canal is less yielding than any other part, and the instrument will certainly pass where it meets with the least resistance; and lastly, I would recommend those who are disposed to follow this practice, previously to inspect the different preparations in this town, showing the frightful deviations which have occurred in the passage of instruments from the route intended."

The following practical details will prove so useful to practitioners, that we insert his own words at full length.

"Metallic bougies are either flexible or unyielding; the former, I believe, are at present but seldom used; they are too little flexible to have any advantage over the elastic gum, and too much so to supply the place of a firmer instrument. The metals or combination of metals of which they are made, are different, but of all metallic instruments, the silver catheter, I think, has indisputable claims to our preference; it admits of an exceedingly fine

polish and uniformity of surface, which very much contribute to facility of introduction; its lightness enables us to judge with the greatest accuracy of the force which the obstacle opposes to its progress, as well as that which we may employ to overcome it: further, we are assured of its entrance into the bladder by the urine escaping through it. The cases in which the plan of dilatation forms part of the treatment, as well as those in which it appears especially applicable, will be described hereafter. The employment of caustic, and of the argenti nitras in particular, is an old remedy revived by Mr. Hunter; and since his time, Sir Everard Home has employed it very extensively. The numerous cases recorded by the latter gentleman, sufficiently prove its powers in removing strictures; but his work, written with a candour well worthy of imitation, contains ample evidence to show that its indiscriminate use is by no means unattended with danger. Hæmorrhages, retention of urine, and intermittent fever, particularly it seems in those who had resided in warm climates, were frequently consequent on its use. The effect of this caustic is to produce a slough which could not be in some cases confined to the stricture; otherwise we are at a loss to account for such profuse hæmorrhage as that which occasionally happened. From the very nature of caustic, it is difficult to confine its action; this we find on applying it to other parts of the body where we have an opportunity of witnessing its effects. The consequences above mentioned naturally induced practitioners to look for some other remedy; many altogether relinquished the employment of the argenti nitras; and its disuse probably became more extensive, in consequence of Mr. Whately's showing us how much more safely we might employ the kali purum; which, in the generality of cases, is equally effectual. However, there can be little doubt that, in relinquishing the argenti nitras altogether, many ran into the opposite extreme. We frequently expect more from a remedy than a cool consideration of its nature would warrant, and when disappointed we as hastily reject it altogether. Impressed as I am with the various occasional inconveniences attendent on the use of the argenti nitras, even in the most skilful hands; and believing, as I do, that in general the kali purum is an equally effectual, and certainly a safer application, I feel obliged to admit, that there are a few cases in which the use of the argenti nitras is to be preferred, and which will be described in their proper place."

This explanation of the modus operandi of caustic potass in the cure of strictures, is exceedingly judicious, as will be seen from the following extract:

"The kali purum was introduced as a remedy for stricture by Mr. Whately; it is certainly a most powerful caustic, yet, by reason of its composition, it can be applied in the manner recommended by him without any

fear of bad consequences. The security in its use arises from the minuteness of the portion employed, and from its being readily miscible with oily and mucilaginous matters. There has been a great deal of speculation concerning the precise mode in which this caustic acts. It is probable that it acts differently in different cases: Mr. Whately speaks of its producing an abrasion, but there is something unintelligible in this term when it is applied to the action of a chemical substance on living parts. The probability is, that it generally acts as a stimulus primarily, whatever may be its ultimate effect; and that, be the quantity ever so small, it occasionally produces a slough; if it be large, it will, of course, do so. Its usual effect in strictures is, however, very analogous to the effect of stimuli elsewhere. Irritable strictures are removed very quickly by kali purum, and in those which are not particularly so, may we not believe, that it stimulates them and produces absorption. It is an admitted principle, that newly-formed parts are incapable of sustaining vehement action; and if this reasoning upon determined facts apply to strictures, which in many instances may be so considered, it readily explains the mode of action by which the kali effects their removal. I have made some experiments with a view to ascertain its precise mode of action when applied to the urethra, but as they were not sufficiently conclusive, I shall not mention them further than remarking, that as far as any inference was warranted, it was decidedly in favour of the opinion that the kali acts as a stimulant."

We pass over his chapter "on the treatment of ordinary cases," in which he recommends the careful use of bougies, aided by appropriate constitutional remedies, and proceed to select some useful remarks "on those cases to which the *kali purum* is especially applicable. The treatment of many cases which are curable by the use of a bougie alone, may nevertheless be happily expedited by the addition of the potass.

"The case, however, in which the kali is so particularly serviceable, is easily known and described—easy of relief by this caustic, but exceedingly perplexing if otherwise treated. In the first place, I would observe, that in those individuals who have been presented to my observation, there has been some very obvious indication of a disordered state of the general health; although the extent of this, as well as its particular feature, has varied considerably. The usual symptoms of stricture are present, and, in general, severe; the peculiarly distinguishing character, however, is a most remarkable sensibility of the stricture. However gently you may pass an instrument —however soft, or, indeed, whatever be its composition, the moment it arrives at the narrowed portion of the canal, the patient complains of severe

pain; and, in those cases which I have seen, blood also invariably follows it when withdrawn. It is this latter circumstance which is calculated to lead the practitioner into error; mere irritability so commonly attends a stricture, and is so frequently relieved by the kali purum, that the excessive degree of it which characterises this case would not alone, in all probability, prevent a surgeon from at once applying this caustic. But a general impression, and so far not an erroneous one, prevails, that if a stricture bleeds, it is more prudent to delay the use of caustic, even where it is proposed eventually to employ it. I was much perplexed by these cases when I first met with them. I used various kinds of instruments; every measure that was calculated to relieve general or local irritation was strictly enjoined, and, as I had reason to believe, implicitly adhered to. Still, however, at the subsequent visit, the same failure in introducing any instrument occurred; and, notwithstanding the gentlest manipulations only were employed, the pain was still considerable, and blood (in some cases to the extent of several ounces) followed after the instrument was withdrawn. Thinking, however, that these were only extreme cases of irritable stricture, and that the ungovernable disposition to hæmorrhage was merely the result of the extraordinary sensibility, I applied the kali one morning to a stricture which was at the time bleeding pretty freely, although the blood did not, in fact, flow otherwise than *guttatim*. The pain during its application was very considerable; yet, as it was not more than that which had been endured from the contact of the unarmed instrument, no part of it was, I think, attributable to the kali. The relief which followed, even within the space of twenty-four hours, was very considerable. Every symptom became diminished in severity, and the painful sensibility completely subsided after two applications; the disposition to hæmorrhage also no longer existed. I have since repeated this practice in every case in which these circumstances were present at the commencement of the treatment, without any hesitation, and have never had reason to regret so doing; the same good effects have been invariably the result. In all cases, the whole of the plan calculated to relieve irritation, has been perseveringly adopted at the same time, therefore, I cannot say what would happen if the kali were trusted to alone; were the surgeon to neglect this part of the treatment, it would be a gratuitous relinquishment of measures well known to be beneficial; so that it is scarcely necessary for me to insist on the expediency of their adoption."

In the cases of stricture where the lunar caustic is useful, there is an extreme want of sensibility in the contracted portion; the rudest manipulations which are usually made, are attended with no pain; neither does any blood follow the introduction of instruments; the severity of the symptoms of stricture bears no proportion to the narrowing of the canal.

The most unpleasant symptom which he has observed is a straining, which is violent; but distressing rather than painful. The sensation imparted to the hand by a bougie, is, as if it was pressing something unusually hard. If the stricture be in the anterior part of the urethra, the latter is felt unusually hard on examining externally. In order to define the characters of this kind of stricture still more clearly, he observes, that it is the very reverse of that for which he recommends the kali purum.

For long strictures, and for cases where the whole length of the canal is contracted, he recommends the use of a silver catheter. He does not approve of the French practice of leaving the catheter in the urethra during any considerable period of time, although he confesses that it has occasionally been attended with great success in his hands. For our parts we must say that we can recommend this plan, from our own experience, as much better calculated to afford relief than the common method of leaving in the instrument but a few seconds, and frequently repeating its introduction.

Under the head of retention of urine, our author recommends to persevere in the soothing plan of treatment (laxatives, leeches to the perineum, anodyne suppositories and warm bathing) until the spasmodic irritation completely disappears. All attempts at introducing instruments before that period, will only serve to increase the irritation, and prolong the sufferings of the patient.

The remainder of the volume is occupied by three chapters —1st, On abscesses and fistulæ in perlneo,—2d, on removal of strictures by incision,—and, 3d, on stricture in the female, followed by the history of ten cases, illustrative of the principles laid down in the preceding chapters. As we have observed nothing of a very novel or interesting character under either of these heads, we will not extend this analysis by a further detail of the excellent practical observations of our author.

M.

ART. VIII.—*Observations on the Use of Colchicum Autumnale in the Treatment of Gout; and on the proper means of preventing the recurrence of that disorder.* By CHARLES SCUDAMORE, M. D. F. R. S. &c. &c. London. 8vo. pp. 116.

WE are pleased with this work, not only as it exhibits the true character of an active medicinal substance, which has for a long time enjoyed an undeserved reputation, but also because its general tenor is a strong and judicious reproof of empiricism generally.

There has never existed any disease, remarkable for the violence of its symptoms, or the degree of its fatality, which has not had its popular remedy. From the earliest ages to the present time, we read of incantations, charms, and material specifics for certain disorders of the human frame, and notwithstanding that the impotency of these have been frequently, we may say continually, exposed, every candidate for empirical fame, still finds a theatre for the practice of his impositions, in the credulity of men.

Gout is one of the diseases which has afforded a large field for the display of empirical ingenuity in the invention of nostrums. Leaving out of the question those remedies depending upon supernatural agency, which we sometimes hear of, we will confine our examination to the medicines which have been recommended by their unfortunate success in occasionally relieving the pain of a paroxysm of gout.

There are some forms of disease in which a neglect of the maxim, that the means of cure should always be directed to the cause, may not be immediately dangerous. But ample experience has convinced us that gout is not to be classed with these. We know that the excrutiating pain of gout may be alleviated in a short time, through the agency of a variety of medicines, but we also know that this relief is transient,—the disease still remains, and will continue to demonstrate itself in successive paroxysms, increasing in violence and in the

frequency of their occurrence. We therefore call the success of such palliatives as colchicum, an unfortunate success, inasmuch as it induces the unhappy subjects of gout to neglect the slow and certain means of eradicating their disease, while they depend upon a remedy which they think will *always* calm its violence, and ultimately terminate its existence. It is in this view only, that we deprecate the use of palliatives in gout. We do not wish to deprive a suffering patient of any means of relief from present pain, but we wish only to destroy that dependence on palliatives which will lead to the neglect of real remedies.

We have, it appears, assumed the fact as granted, that gout is not to be cured by those means which are usually resorted to for the purpose of temporary relie. In this position we are supported by almost all the respectable testimony on record.

This history of the disease, indeed, is sufficient to convince us of the fallacy of depending upon any sudden means for its removal. It is an evil of deep root and gradual maturation. It is the result of long continued improper habits of living, and frequently dependent upon hereditary predisposition. In either case it were folly to attempt its removal by any other means than those which effect a permanent change of the system generally, and this, we know, cannot be reasonably expected from the sudden influence of any specific stimulus.

Our author very properly observes, that "gout, even in its first visitation, is a compound disease, external in its appearance, internal in its cause. The paroxysm often takes the patient by surprise, and finds him in the enjoyment of health; or more correctly speaking, possessing comfortable feelings, and those good looks which give the portrait of health; although, most assuredly, the constitution is not in a healthy state."

"I am now giving a general description, and claim a licence against the exceptions to it, which occasional and individual example would furnish. In this general view, therefore, I

proceed to observe, that the kind of constitutional error which attends a first fit of gout, is an overcharged state of the vessels which belong to the abdominal viscera, and chiefly in the vessels connected with the liver; the secreting action of this important organ being also in some way altered from its usual state. The stomach itself is the organ least deviating from its ordinary and healthy state. The appetite very commonly continues natural, unless, indeed, the symptoms are sufficiently severe to produce high sympathetic fever, when thirst and loss of appetite would naturally follow. The stomach, it is true, has been the parent source of gout, in being the medium through which too much animal material has been introduced into the system; but it seldom suffers much inconvenience in the first instance.

"In the progress of time, if improper habits are continued, dyspepsia, or true indigestion, will be produced. Most commonly we find that the patient traces a gradual enlargement of the abdomen, joined with various indications of an increasing fullness of habit. The veins, especially of the extremities, are more distended with blood than is natural; and there is a want of due softness and pliancy in the right hypochondrium, where the principal part of the liver is situated. There is too much abdominal corpulence; the secretions are changed from their natural state; the intestinal evacuations are darker than usual, they have a morbid degree of fœtor, and lead us to the conclusion that the bile has acquired an irritating and acrimonious quality. I infer, under such circumstances, that the liver, receiving an excessive supply of blood from that important vessel called the vena portæ, is stimulated into increased action; and not only is a larger quantity of bile formed, but it is also more exciting in its quality. That the process of assimilation on the conversion of chyle into blood is disturbed from the healthy course, is most commonly proved by the condition of the urine, which has a higher specific gravity than usual, is strongly animalized, and deposits dense sediments."

This picture of the disease readily points out its remedy. There is extensive organic derangement, and consequent interruption of function upon which the more prominent, or, as our author expresses it, the external symptoms of gout depend. These derangements have been gradually effected; they must, consequently, be gradually corrected. On this subject, however, we will have occasion to speak hereafter; we postpone it, at present, to confine our remarks to the use of colchicum.

Dr. Scudamore mentions the *eau medicinale* as a remedy for gout, whose introduction formed an era in the treatment of that disease. That this preparation did, in very many instances, succeed in removing paroxysms of gout, is not denied; indeed, its prompt agency in removing present symptoms, was such as to insure it very extensive popularity. "In its first trials," says our author, "in the majority of instances the patient seemed to obtain a cure as from a charm; so speedy and magical was its influence. But the fallacy of the apparent cure always appeared sooner or later; and the frequent failure of the remedy to produce even temporary relief, with the very serious and even irreparable injury often inflicted on the constitution, in the course of a few years, brought the true character of the medicine to light. Meteor-like, its undeserved fame ere long declined, and has now wholly passed away."

The *colchicum autumnale* is now introduced as the successor of the *eau medicinale.* It may be proper to remark, *en passant*, that the colchicum is not a new remedy, but that it appears to have been used by Demetrius Pepagomenus, in the thirteenth century, and was known at that time by the name *hermodactyle.*

"From the comparative trials that I have made," says Dr. Scudamore, "of the powder of the root, the tincture and the acetic preparation, I soon became convinced of the remarkable mildness of the latter, and the preference due to it in that particular. By joining it with magnesia and a neutral salt,

such as the sulphate of magnesia, I found a combination inoffensive to the stomach, and remarkably certain in its operation on the bowels. I was at a very early period impressed with the conviction, that the most favourable use of the colchicum consisted in making a selection of this, the mildest preparation, and of employing it only in combination with other medicines of an aperient and corrective nature. It was my object to borrow, in a safe manner, from the power which this medicine might exert over the symptoms of the disorder, but not to regard it as a lasting agent of cure."

Our author proceeds to mention "Wilson's tincture" and "Reynolds' specific," both of which operate similarly to colchicum, and are, probably, preparations of that article. Of the first of these he observes, that Wilson's tincture operates in the same manner as the *eau medicinale*, but must be given in larger doses. It disagrees with the stomach, and produces giddiness and other unpleasant symptoms. "I have known the strongest persons rendered very nervous from its influence; sometimes in a fearful degree. In a few instances it has continued to give relief to the symptoms without any apparent abatement of its efficacy; but such favourable examples are rare, and I do not know any instance in which it has not weakened the nerves more or less."

Dr. S. relates two cases in illustration of the opinion which he entertains of the character of Wilson's tincture. They would no doubt be quite in point if we could attach any specific meaning to such phraseology as "*weakening the nerves*," which is mentioned as one of the evil effects of colchicum. There are so many symptoms of disorder referable to a variety of morbific causes, that are, in common parlance, attributed to "weakness of the nerves," that we hope our author will excuse us for this passing criticism on his language.

We do not copy the cases by whose history Dr. Scudamore illustrates the effect of Wilson's tincture, nor do we think it necessary to follow him in the specification of the slight

shades of difference that may be observed, by close attention, between this remedy, Reynolds'specific, and the acknowledged preparations of colchicum. The general result of his experience is, that the *eau medicinale* is the most effectual, of the various remedies above mentioned, in removing a paroxysm of gout, and that it acts in smaller doses than the other preparations—next to it follows "Wilson's tincture" in point of potency, in which particular it is a little superior to "Reynolds' specific:" and of the various preparations of colchicum, the proof spirit tincture is the most active, though the acetic preparation given with neutral salts is the most expedient. Finally, that no dependance can be placed upon any of these substances as remedies; on the contrary, that in some instances they have aggravated the disease, and finally, that all the advantage which can be derived from them is confined merely to the object of procuring transient relief from the anguish of a present paroxysm.

Having destroyed our hope in this quarter, our author has pointed us to a more rational source, whence to derive exemption from this attendant curse upon the indiscrete indulgence in the luxuries of life. We take great pleasure in following Dr. S. through this part of his subject, as we think that his observations are the result of true experience and sound medical reasoning, and are, therefore, calculated to be extensively beneficial. We would, indeed, be highly gratified in being in any degree the means of "holding the mirror up to nature," so as to show many who suffer in mind, body and estate, from this protean disease, the real source of their grievances, and the only method on which they can reasonably depend, for a happy deliverance out of their sufferings.

From the observations on the nature of gout, introduced at the commencement of this article, we are prepared to expect the recommendation of some other plan of cure than that which has for its object the sudden removal of pain and local inflammation.

According to the maxim, that the first step in the cure of a disease is the removal of its cause, in those cases of gout which result from indulgence in rich food and sedentary habits, the necessity of a change in these particulars is sufficiently evident.

Excess is always dangerous, and should therefore be avoided. It is a nice question to determine whether a sudden transition from the simple food and active life of the labourer, to the indolence and luxury of the epicure, would be more immediately pernicious, than the sudden change from high living to excessive abstemiousness. As evil consequences are to be apprehended from any sudden and excessive change of habits, we must allow this consideration to modify the practice which is indicated by the maxim above mentioned. The change, therefore, to be made in the habits of the patient, must be gradually effected, independently of the degree to which it must ultimately be carried "The experience and attentive observation of many years," says Dr. S., "have convinced me, that it is highly important that the cure of gout should be gradually conducted."

The treatment which our author recommends, consists chiefly in the use of aperient medicines, and the occasional use of calomel as an alterative. By these means, conjointly with a gradual relinquishment of unnecessary stimuli, those visceral congestions, upon which the disease mainly depends, are removed. These principles are illustrated by a case which we give in our author's words.

"A gentleman, considerably advanced in years, had been in the habit of taking the different strong medicines, the *eau medicinale*, Wilson's and Reynolds' medicines, and the wine of colchicum, in succession, accordingly as the fame or accidental recommendation of one remedy had superseded that of another. Instead of being attacked by regular fits of gout once or twice in the year, he was assailed by the disease every four, six, or eight weeks, which had brought upon the constitution every imaginable distress. I found the bowels in a most

unhealthy state, and although the appetite was in general sufficient, the digestive powers and the ulterior process of assimilation had become so impaired, that, to use the impressive language of the patient, all his good food seemed to turn only to the disease and to his discomfort.

"I commenced my plan of treatment solely by the use of correctives, and aperients, and regulated diet. When gouty action returned, I employed the *acetum colchici*, neutralized and in combination with aperients, with every advantage; but except during the prevalence of the irritation of gout, I did not use even this mild preparation. The limbs were sponged night and morning with a lotion of camphor mixture and alcohol, used at a tepid temperature.

"The second stage of treatment consisted in combining the use of tonic with alterative and aperient medicines; and as soon as the chronic inflammation and tenderness of the joints were removed, sponging with tepid salt water and moderate friction, and champooing were employed for the limbs.

"The concluding treatment comprehended the discontinu- of alterative medicine, the diminished use of aperients, occasional use of tonics, and continued attention to the limbs; for the treatment of which, a stimulating liniment was used. Change of air, horse and foot exercise, and all general aids to health and spirits, filled up the plan for restoration, with such success, that two years passed away without a return of gout; and in the third year, one paroxysm only occurred, which was short in duration, and yielded readily to my usual plan of treatment."

Other cases are related by our author, illustrative of the nature and proper method of cure of gout; but, as their history does not materially differ from that which we have just quoted, we do not think it necessary to enter into their details.

Dr. S. is not unwilling to take advantage of the occasional benefit to be derived from colchicum and other substances calculated to remove a paroxysm of gout. But the following

extract will show how little dependance he places in them, as exclusive remedies:

"The fundamental principle of my objection is, the impropriety and danger of arresting the symptoms of gout, by means of the strong agency of these specifics, without producing any useful and adequate influence on the cause of the disease.

"Let it be constantly kept in view, that all which appears externally, and to which the name of gout is given, is but the least part of the real disease; and that the cause exists in the system within. In its first occurrence, the gout is a much more simple disease than afterwards, and most commonly the result of a particular kind of repletion, belonging to the abdominal viscera. The patient has found himself growing corpulent, particularly in the abdomen, and has acquired a fullness of habit; the excretory functions have been inert; and these circumstances concurring with the peculiar disposition to this disease, a paroxysm takes place.

"In the course of years the nervous system yields more or less to the dominion of gout; and it will often happen, that a fit shall be produced *by high nervous irritation*, in consequence of mental or corporeal causes strongly affecting the nervous system. The strong passions of the mind, injury to the body from accidents, the disturbance produced by other diseases, have this influence.

"In general it happens that the predisposing condition of the constitution, which terminates in a fit of gout, has been gradually forming for a considerable period of time. A state of disordered function in the several abdominal viscera has been insidiously taking place, of which the patient has been wholly unconscious.

"Even as a matter of theory, it would appear quite unreasonable to attempt the cure of a disease thus slowly formed, although suddenly appearing, by means of the powerful agency of a single dose or a few repeated doses of any medicine; and it would also suggest itself as a probability that it

would be attended by injury to the constitution, to dismiss the symptoms so quickly, while the cure of the cause could not possibly be accomplished in so summary a manner."

"Some persons who have taken the alarm from a first visit of the gout, and who have had the resolution and prudence to adopt a considerable degree of forbearance in their habits of living, and care in their general regimen, have been successful in preventing a return of the complaint. These have been principally instances of acquired gout; but when the disposition is hereditary and very strongly existing, it must be admitted that every care will not entirely counteract the return of the disorder. Yet the value of such care is not to be the less esteemed, for it will serve most materially to mitigate and diminish evils which cannot be altogether prevented.

"The gout, then, is a disease prone to return more or less frequently, to increase in severity, and to effect a great number of parts, as the constitution becomes more subjected to its decided influence.

"Even in its simplest form, as exemplified in a first fit, I have already stated that it is connected with that species of repletion which belongs to the vessels of the abdominal viscera, and chiefly of the liver. But in its progress, it manifests this connexion much more strongly; and I do affirm that, in every long-established case of gout, the functions of the liver are more or less unhealthy; and, in a very large proportion of cases, the symptoms of gout are entirely supported by a great derangement of the biliary system, and by an unhealthy condition of the intestinal canal. This disorder of functions is rendered manifest by the appearance of the excretions."

As Dr. Scudamore is of opinion that in gout the liver is seriously affected, we are somewhat surprised to find in the division of his work devoted to the "use of mercury," the following opinion: "I consider it an axiom of importance, that mercurial medicine should never be administered to gouty persons, to any extent which carries with it the risk of producing mercurial fever." We cannot see why the mercurial

action should be dreaded in this disease, more than in other circumstances attending a disordered liver, where we depend almost entirely upon the specific action of mercury.

Our author, however, has some faith in calomel, employed for other purposes than the production of mercurial *fever*.

"When the alvine excretions are morbidly dark and fœtid, and the urine deposits a lateritious sediment, I give the preference," he says, "to a moderate dose of calomel, combined with about a grain* of James' powder, some compound extract of colocynth, and three or four grains of extract of poppy, to be administered at bed time, directing a suitable aperient to be taken in the morning, before rising.

"This method being continued, with occasional intermissions, till the appearances afford the evidence that vitiated secretions are effectually removed, it will be expedient to use the more alterative form of mercury, as, for example, the pilula hydrargyri, the hydrargyri oxydum cinerum, or the pilula hydrarg. submur. comp. in combination with the compound extract of colocynth, or the extract of rheubarb; giving, regularly, a gentle aperient in the morning, of a nature not calculated to weaken the stomach, or produce nausea. Half a grain or a grain of ipecacuanha, or a grain or two of castile soap, may, on some occasions, form an useful addition. I prefer the ipecacuanha, if desiring to increase the secretion into the bowels,—the powder of squill, if wishing to act on the kidneys,—and James' powder, for the purpose of promoting a more free action of the skin.

"When the biliary functions are completely restored to health, the mercurial alteratives should either be laid aside, or used only occasionally; but when the action of the liver or the intestinal canal is permanently in error, the value of

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* We confess that we cannot calculate the effect of one grain of James' powder, from any data, that we possess, respecting the actio of this compound.

mild doses of mercurial medicine, in conjunction with aperients, is, in my mind, unquestionable. The alterative administered at bed time, and the aperient before rising in the morning, afford real benefit and sensible relief. In this manner also an increased action of the bowels may be maintained for a long time without producing debility; because the morning medicine does not rob the patient of nutriment, while it insures the removal of vitiated accumulation from the bowels.

"I am solicitous to draw the just line of distinction between the use and the abuse of mercury. If employed with judgment and caution, it is the most valuable remedy in the materia medica, for effecting the desired change of action in the different secreting organs, connected with digestion: and I confess my regret, that any respectable author in medicine should level accusations against almost any use of mercury, which can only, with the smallest propriety, be offered against its abuse.

"In gouty persons, and those who are not liable to gout, we meet with examples of constitutional indisposition, so obscurely marked, that, unless we investigate the nature of the excretions from the alimentary canal, in a very regular manner, we will fail to obtain any knowledge of the nature of the case, or its proper treatment."

Our author here makes an allusion to a form of disease, not, we believe, very common in its occurrence, but of very obscure and difficult diagnosis. It is stricture of the rectum, sometimes spasmodic, in other instances permanent. It is marked by irregularity of the bowels, particularly costiveness. Active cathartics at one time fail to produce their proper effects: again, the bowels becomes very irritable, and the most unpleasant feelings harass the patient. The treatment which is recommended, is the use of the bougie.

Our author has not neglected a very important consideration in the treatment of gouty patients, namely, attention to diet. We perfectly agree with him in the opinion, that "a strict attention to suitable regimen, in all its particulars, is so

important, that, if neglected, neither physician nor patient can obtain success."

This is so important a consideration, and so judiciously urged by our author, that we cannot refrain from extending the present article to a few observations in relation to it. We believe, indeed, that the progress of disease, generally, would be diminished in a much greater degree by proper attention to diet and prophylactic habit, than by any dependance upon counter-agents to morbid action already formed.

"Although it happens occasionally that the utmost care and prudence prove insufficient to prevent the first visitation of the gout, or its returns, yet it is still more familiar to our observation, in how great a degree the disease is the offspring of indulgence and irregularity in the habits of living.

"Selection in the articles of diet, and moderation in quantity, must be the peculiar care of the gouty patient, if he desire to counteract the returns of the disorder. As regards the quality, he must be mindful to avoid those materials which most tend to cause acetous fermentation; also, on the other hand, those which are too nutritious. A dinner of turtle might be digested with perfect comfort, but it would be a deviation from the proper principle of diet, as tending to the formation of too much blood.

"Until the constitution has become weakened, it usually happens that the stomach of a gouty patient is very competent to its office, and that the appetite is regularly active rather than deficient. In fact, it is a very common case, that too much food is taken in the stomach, to admit of the subsequent process of digestion being perfectly performed; or, supposing that every stage of the digestive action is completed in a healthy manner, and that all the chyle is assimilated into good blood, a plethora gradually takes place, which introduces a fit of gout.

"Indigestion in the stomach is readily distinguished by the patient, in the notable symptoms of heart-burn, flatus, a sense of weight, or soreness, after a full meal: but the consideration

of diet, as respects the plethoric state of the constitution, belongs to the exercise of judgment and reflection, rather than to sensation.

"Common prudence and immediate comfort, would dictate to every one, the necessity of avoiding any kind of food which experience has proved to be unsuitable. In principle, also, many articles of food should be avoided by the gouty patient, and, as being unfavourable to immediate digestion, I may mention,—salmon and stewed fish; pork food, unless care be taken to avoid the skin and fat; all meat which is not tender, but especially hard salted meats; pickles, unless occasionally and sparingly, and when they are well seasoned; salads, except now and then, and when the stomach is in the best state; for, besides that, these latter articles are not convenient to a stomach liable to become disordered, they quicken the appetite too much for meat. Rich pastry, and most confectionary should be avoided; although a plain pudding, with any kind of good fruit, or the preparations of Semolina, rice, or bread, and stewed fruit, are as proper for a gouty individual as any other.

"Most persons commit still greater error in the quantity, than in the quality of their food; and, as respects the production of gout, it is eventually even more important. A fit of indigestion will often cure itself: but the gradual repletion of vessels which ensues from a full system of diet, with certainty leads to a continued disease. Hence it is of consequence to avoid heavy, rich soups; and as a general rule, I would advise the restriction to plain well-made gravy-soup, and that sparingly.

"Simplicity of diet consists greatly in choosing what is most proper, and confining the meal to such judicious choice, instead of partaking of a great variety of dishes; but the temptations of a well spread table, demand the exercise of greater prudence than is usually practiced."

In proportion as care and moderation in diet are observed, together with a due share of regular exercise, the use of medi-

cines is less required. Important to health as the regulation of the bowels is for all persons, it more particularly concerns the gouty patient. The natural action of the bowels, however regular, does not appear sufficient to prevent the accumulation of morbid secretions; which, after a time, seem to concur with other circumstances in giving rise to a fit of the gout. Experience is in favour of this supposition: we find that the frequent use of aperients tends materially to prevent the recurrence of gout.

"I would lay it down, as a general rule," says our author, "that every gouty person, however regular the state of his bowels may be, should take some aperient, at least once a week, throughout the year, and if he be corpulent and of full habit, twice a week; and farther, that in every instance it is important that the bowels act once a day, as a constant rule; and those who are corpulent, or much prone to gout (unless debilitated) should study to produce the habit of action night and morning."

In the close of his work our author expresses his conviction that there is no disease more perfectly within the power of medicine than gout. The plan of treatment of which we have given a sketch, is certainly founded in a correct knowledge of the pathology of this disease; and the slight variations which may be demanded by peculiar circumstances will be easily suggested to the intelligent practitioner, who attends to the principles which we have attempted to display.

T. M. H.

ART. IX.—*Report of the Transactions of the Academy of Natural Sciences of Philadelphia, during the year* 1824. Submitted by the Recording Secretary in pursuance of a Resolution of the Academy. Dec. 1824.*

INDEPENDENTLY of the real utility which may arise from the cultivation of natural history, with regard to our wealth and comfort, the peaceful walks of the naturalist invite us, by innumerable attractions, to indulge our more amiable feelings and forget the scenes of "noisy folly and discordant vice," which continually beset us in our commercial and political connexions.

From those turbulent occupations in which the jarring interests of men exhibit so many pictures of disgusting selfishness and unruly passions, we turn with delight to the contemplation of the works of nature, whose infinite variety affords an endless succession of novelty. And while we admire the splendor of some of her works, and the inimitable beauty of others, and the harmonious order which reigns throughout all, we cannot but adore the wisdom, and tremble at the power which gave existence to the earth, and stored the ocean and the land with innumerable animated beings, and not less wonderful inanimate objects, all conspiring to the support, to the instruction, and to the happiness of intellectual man.

As a celebrated philosopher observed, "the undevout astronomer is mad," so with equal propriety may we pity the ignorance and insensibility of those who ascribe the labours of the botanist to "puerile enthusiasm" and who see, in the magnificent science of geology, nothing but the "useless art of giving names to stones."—But civilized society is daily enjoying the advantages, both intellectual and commercial, which have arisen and continue to flow from the cultivation

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* We are indebted to the politeness of the editor of the Port Folio, for whom this article was prepared, for the privilege of inserting it in this journal. ED.

of the natural sciences: and while the older and more polished nations of Europe are vieing with each other in the enjoyment of well-earned honours, for their proficiency in these pursuits, it is gratifying to see in our young nation a spirit of emulation and enterprising zeal in the pursuit of knowledge, and for the promotion of general good.

The institution, a report of whose transactions is at present before us, although of recent date, has acquired a respectable and highly interesting character. It has been the means of developing much native talent, and making many additions to our knowledge in the various branches of natural science; some of which, especially in the department of minerology, may become very important to our commerce and manufactures. The investigations of the zoologists and botanists of the academy, have enriched these provinces by the introduction of many new genera, species and varieties, the products of our extensive region, and they have also corrected many errors of older professors in the classification and nomenclature of natural objects.

In the department of zoology, the well known names of Lesueur and Say, appear as contributors to the academy, during the last year. We copy, *en passant*, a judicious remark of the former gentleman, respecting nomenclature. "I have adopted for this species (cephalopterus *vampyrus*) the name of the celebrated Giorna, well known in science, and reject such names as Devil, Vampyre, &c. which may be associated together as names calculated to repel those who are disposed to admire the beauties of nature, and who have an inclination to cultivate scientific and natural history. How far preferable is the custom of applying the names of those naturalists who have enriched science with their discoveries, or new and valuable observations, to that which introduces into our pages those chimeras which exist only in a morbid and timorous imagination!"

We may take this occasion to remark, that too little attention is paid to nomenclature, in most of the natural sciences.

In the infancy of a science, the want of a correct nomenclature, founded upon immutable principles, is not much felt; but when the objects of study become multiplied, both in number and in diversity of character, a distinct classification becomes absolutely necessary to the acquisition of a knowledge of them.—Zoology, botany, chemistry and mineralogy, present us with numerous examples of a plurality of names applied to one object, which unphilosophical practice occasions great perplexity and discouragement to the student. Botany is peculiarly liable to this unhappy obstacle to her advancement, by reason of the variety of sources from which the name of a plant may be suggested—namely, peculiarity in the figure of the flower, the leaf or the stem, its habits, the country in which it is found, and the name of its discoverer. The other sciences are confined within more reasonable bounds. Chemistry, particularly, enjoys a great advantage over all the others, in the possession of a nomenclature, which not only prohibits the slightest departure from established rules, but also recognises in its specific appellations the nature and constitution of its subjects. But to return to our more immediate subject.—Mr. Say has made many interesting additions to the entomological collection of the Academy of Natural Sciences. In exploring our continent, between the valley of the Mississippi and the Rocky Mountains, this gentleman was enabled to collect a large number of insects before unknown to entomologists. Among his contributions to the academy, for the year 1824, it is observed in the "Report, &c." that he has added no less than forty new species to the order hemiptera.

The Recording Secretary's Report, takes notice, also, of other contributions from this gentleman, whom we have named, and from Drs. Harlan, Troost, Pœppig, Mr. Bonaparte, and others.

As it is foreign to our purpose, and incompatible with our limits, to give a detail of the subjects of the various communications mentioned in the "Report," we refer those who

are interested in these matters, to the journal of the transactions of the academy, several volumes of which have been published.

We take this occasion to invite the attention of gentlemen who reside in the country, to the objects of this society. Our country has already yielded many interesting and useful additions to every department of natural history, and there is no doubt that it yet contains a variety of objects, a knowledge of which is very desirable. Instances frequently occur, of interesting additions being made to our catalogue of minerals and organic remains, from the collections of gentlemen, who, although not versed in science, have been stimulated either by a taste for natural history, or a very laudable curiosity, to preserve such specimens as appear to them to be uncommon. It is particularly desirable that fossils and organic remains, wherever dicovered, should be preserved, with an accurate description of the local situation in which they may be found. Many of the objects of natural history have become known only by the discovery of their skeletons; their race having become extinct; and some of these, for example, the mastodon, or mammoth, are highly interesting.

T. M. H.

ART. X.—*An Essay on the Symptoms and History of Diseases; considered chiefly in their relation to Diagnosis.* By MARSHALL HALL, M. D. F. R. S. E. and formerly senior president of the Royal Medical Society of Edinburgh. Octavo, pp. 130. London. 1822.

THAT a knowledge of the nature and particular signification of symptoms is of the utmost importance to the successful practice of medicine, is too obvious to require any argument. He who is deficient in symptomatology, will always grope in the dark, and prescribe under disadvantages which no other information can supply. As a distinct and particular department of study, however, semeiology is unaccountably neglected in the English and American universities. On

the continent of Europe, works on this subject are numerous; and in most of the medical schools of France and Germany, particular professorships are allotted to it. Until the present work of Dr. Hall appeared, there existed none in general circulation in the English language, to which the mere English reader could refer. The medical public are therefore under peculiar obligations to Dr. H. for this volume, and the more so as he has treated the subject in a very clear and edifying manner.

Chap. I. *Countenance.*—The expression of the countenance—or, as it has sometimes been called, the physiognomy of disease, when carefully studied and well understood, will often aid the practitioner very materially in forming a correct opinion concerning the nature and tendency of diseases. Independently of the changes of colour which occur in the skin, from deranged functions of the digestive and biliary organs, or from the peculiar condition of the capillary circulation, local congestions, &c. there is, in almost every form of disease, a peculiar expression of countenance, which serves as an important index to the experienced practitioner.

"Among the particular features, it is of moment to observe the eye, the prolabia, the brow, the nostrils, the lips, &c. The eye, in particular, affords the opportunity of judging of the degree in which the serum is loaded with bile in cases of icterus, and of distinguishing that disease from those morbid affections in which the complexion becomes sallow and icterode, from the state of the cutis and cutaneous circulation. The state of the prolabia affords an index of other states of the blood—as of a too serous condition, or of a defective arterialization. The nostrils, carefully observed, denote the condition of the respiration."

The author next points out those appearances which the face presents in particular affections. In *common acute fever*, he observes, the countenance exhibits a diffused flush, attended often with a turgidity, particularly in young and plethoric subjects. The tunica albuginea is generally suffused, there

is tremor of the lips in speaking, and the nostrils move rapidly in respiration. "In *acute symptomatic fever*, the countenance has a very different aspect, which it is important to observe, especially in a diagnostic point of view." There is, in this form of fever, no turgidity and flushing of the face; no suffusion of the eyes; no tremor of the lips, and hurried movement of the nostrils in respiration; and whilst these symptoms are absent, the surface is frequently affected with perspiration.

"In chronic symptomatic fever, the appearances are peculiar. There is a characteristic expression of disease which strikes the common observer, and, still more, the experienced physician; the surface and complexion are cool and pale, or affected with transient or partial heat and flushing, usually without sallowness, frequently with slight lividity, sometimes with cool moisture; there are emaciation and shrinking, the cheeks falling in, the action of the muscles becoming apparent, and the skin forming into greater or smaller folds. These appearances of the countenance are, however, greatly modified by the nature and seat of the original disease, as will be particularly noticed hereafter."

In disorders of the digestive organs, particularly when severe, there is always a paleness and sallowness of the face, with a dark hue about the eye; "the cutaneous vessels exude a little oily perspiration; the prolabia are slightly pale and livid; the muscles of the face, and especially of the chin and lips, are affected with a degree of tremor, particularly on hurry or surprise, or on speaking." This character of the complexion, &c., usually attends dyspepsia.

The icteroid appearance of the countenance in chlorosis, observes Dr. Hall, should not be confounded with *icterus* itself.

"The term *icterus* is merely expressive of a *symptom* of disease, although it is daily named, and has long been arranged as a distinct *disease*. The shade varies from yellow to green, or blackish. But the most important and only *practical* dis-

tinction, with regard to icterus, is that of its *causes*, or of the *primary disease;* the principal of these are:—1. Constipation, or loaded bowels:—2. Acute disorder of the digestive functions:—3. Disease of the liver:—4. Gall stones:—5. Hydatids in the gall ducts:—6. Organic tumours in the abdomen, situated near the biliary ducts:—7. The pregnant uterus:—8. Disease of the right kidney:—9. Or even of the right lung or cavity of the pleura."

We shall not stop to notice our author's observations on the expression of the countenance in apoplexy; but the remarks contained in the following quotation are particularly worthy of attention:

"In inflammation of the chest with acute pain, or pleuritis, the degree of the pain is marked by a proportionate contraction of the features in general, and by acuteness and elevation of the alæ nasi; the nostrils are moved and dilated by the alternate acts of respiration; there is sometimes a degree of vivid flushing, terminating abruptly, and bounded by whiteness towards the nose; the heat is inconsiderable, and there is frequently perspiration.

"*In inflammation of the chest, with dull pain or pneumonia,* there is less constriction of the features, but there is an appearance of anxiety, and the nostrils are widely dilated before each inspiration; there is a little heat, but frequently a degree of perspiration.

"*In inflammation of the chest with great dispnœa,* and in cases of effusion into the lungs, there is usually a general and deep suffusion of the countenance, sometimes amounting to great lividity, and conjoined with turgidity; there is great anxiety, the nostrils are widely dilated on inspiration, and drawn in above the lobes; during inspiration, too, the pomum adami, and even the chin, are sometimes drawn downwards; the surface is cool and sometimes damp.

"The dawn of *phthisis pulmonalis* is marked by a delicate paleness, alternated with transient gentle flushing, slight lividity of the prolabia on exposure to cold, an appearance of

indisposition, frequently motion of the nostrils from the respiration, and frequently a quivering of the chin and lips in speaking. Its progress is denoted chiefly by gradual emaciation, in addition to an aggravated state of the other morbid appearances just mentioned.

"In hæmoptysis there is usually a florid state of the complexion, and frequently the effects of dyspnœa are observed in an acuteness and movement of the nostrils. If the hæmorrhage has been very great, there may be paleness, lividity, coldness, and a clammy perspiration, with great anxiety."

"In *inflammation of the abdomen with severe pain*, there is a continued state of the contraction of the muscles of the face, inducing an unnatural acuteness of the features; the forehead is wrinkled, and the brows knit—the nostrils are acute, drawn upwards, and moved by the alternate and irregular acts of respiration,—the wrinkles which pass from the nostrils obliquely downwards, are deeply marked,—the upper lip is drawn upwards, and the under one perhaps downwards, exposing the teeth,—the chin is often marked with dimples. This state of the features is aggravated on any increase of pain—from change of position by muscular effort, or from external pressure. Indeed, in cases of abdominal affection, it is better to press on the abdomen, or beg the patient to raise the head and shoulders, and *watch* the *effect* on the expression of the countenance whilst the patients' mind is occupied with some other subject than to ask the direct question whether pressure induces pain—as is usually done; for patients naturally suppose that every painful part must be also tender, and are therefore apt to answer in the affirmative, although incorrectly."

Chap. II. *Attitude.*—By attitude Dr. Hall means, "*the posture and motions of the body, the state of muscular debility, power, contraction, and motion, some particular actions, and the general manner of the patient.*" According to the observations of our author, *tremulous motions*, and a *supine posture*, distinguish acute idiopathic from symp-

tomatic fevers in their early stages. Retention of the urine, in febrile diseases, may be suspected when the patient is constantly keeping his knees in an elevated position. The author's observations on the positions usually assumed in asthma, painful abdominal inflammation, colic, gangrene, strangulated hernia, and nephritis, are precise, and deserve to be attentively studied. In *organic diseases*, the patient soon becomes affected with permanent debility; he stoops in walking, and moves with slowness and caution. "Deep-seated pain or uneasiness is often produced by the succussion induced by *sitting down*, or making a false step in walking. In the appearance of the hand it is often easy to read a state of pain, anxiety, or other suffering,—it is closed or expanded, or variously moved."

"There is another symptom," observes Dr. H., of importance to be noticed, viz. "the state of *jactitation* and inquietude; it occurs in different states of the system, and in some diseases, but principally in cases of *irritation, exhaustion*, and *sinking*, and in *diseases of the heart.*"

"General convulsion," says Dr H., "is more particularly observed in diseases situated about the *base* of the brain."

Tongue.—This organ is variously affected in diseases. It becomes swollen, indented, fissured, and lobulated, and its surface covered with whiteness, fur, blackness, dry brown crust, chaps, &c. In some cases its papillæ are preternaturally prominent and enlarged; and in others almost entirely obliterated, giving its surface a smooth shining appearance. Sometimes it is protruded with great difficulty, owing to paralysis, dryness, and tremor; and occasionally also we find the patient incapable of retracting it, particularly where there is great insensibility. Dr. H. very correctly remarks, that the appearances of the tongue afford the best diagnostics of the various forms of idiopathic fevers, and of idiopathic from symptomatic fevers.

The following is an excellent description of the various appearances which the tongue assumes in dyspepsia, from its simple to its most aggravated form:

"The most ordinary effect of an occasional or accidental derangement of the stomach and bowels, is a loaded state of the tongue, the superior surface of this organ becoming covered with a layer of whitish, soft, mucous substance, admitting of being partially removed by the tongue-scraper; the whole internal mouth is, at the same time, more or less disagreeable and clammy, the taste depraved, and the breath offensive; and frequently the substance of the tongue is a little swollen, œdematous, and marked by its pressure against the contiguous teeth. In *acute dyspepsia*, the state of the tongue, already described, is observed with some modifications:—the tongue is in general loaded, the mouth clammy, the taste bitter or nauseous, the breath fetid, whilst the surface of the face is frequently oily; in some severe cases, the *load* has been very thick, and has eventually peeled off, leaving the tongue red, smooth, and tender; the *substance* of the tongue is generally swollen, œdematous, and impressed by the contiguous teeth: the *gums* are often red, tumid, and somewhat separated from the teeth by tartar, and are easily made to bleed; the inside of the *cheeks*, also, frequently partakes of the œdema, and receives, like the tongue, impressions from the adjacent teeth; sometimes the cheeks and the gums of the posterior part of the mouth have been so swollen as to protrude a little over the teeth, and are either *ulcerated* by the pressure, or *wounded* by being bitten—circumstances which are apt to be induced or aggravated by cold. Through the load on the tongue the red papillæ are frequently seen either over its whole surface, or at its point principally; frequently the tongue is not only *indented*, but formed into *creases* or folds; sometimes deeper and more numerous sulci are formed, the edges of which are sharp and the sides in contact, and requiring to be separated by the two fingers, or by protruding the tongue further; in some cases the tongue is

less loaded and indented, and its edges are red and even. In cases of acute dyspepsia, I have seen the tongue affected with deep, foul ulcers, resulting from the slow suppuration of a hardness about the size of a horse-bean or nut, situated just under the surface of the tongue, which is loaded, swollen, and foul, with a copious saliva and a fetid breath. In very *protracted* cases of chronic dyspepsia the tongue assumes several remarkable modifications of form and surface:—in the *first* case, there is a universal enlargement of the papillæ over the surface, which is now generally clean; in two instances the papillæ at the *most posterior* part of the tongue became particularly enlarged, causing pain on swallowing, and some alarm to the patients; in the *second* modification, the surface of the tongue is formed into lobules, sometimes deeply intersected, and resembling in form those of the base of the cerebellum; at other times, of less regular form; and lastly, assuming the form of squares; in the *third* variety, the tongue acquires an absolute and morbid smoothness of surface, which appears as if glazed, and is tense and unyielding. In all these cases the tongue is morbidly clean, the mouth, taste, and breath, being nearly natural, and its colour, although perhaps rather paler, frequently little changed."

The appearance of the tongue in chlorosis is quite characteristic of this disease; in the commencement of the disease, the tongue is pallid, somewhat tumid, slightly loaded, indented, and furrowed; the gums and prolabia also are pallid, and the breath is foul. As the disease advances, the tongue becomes more and more deprived of blood, and acquires a peculiar semi-transparent appearance, and a very pale lilac hue. The papillæ disappear, and the surface often becomes smooth and shining. The same pale hue is discovered in the gums, prolabia and inner surface of the cheeks. The breath acquires the odour of *new milk;* in the advanced stage of the disease, however, the mouth is less clammy and disagreeable than during its early periods.

Dr. H. observes, that there is, in this complaint, an affection of the prolabium and surrounding skin, which has not been noticed by authors. This is a peculiar dry-splitting and exfoliation of the cuticle of the parts surrounding the mouth, to the distance of about one-fourth of an inch.

Chap. IV. *General Surface.*—We shall pass over this chapter, as it does not contain any observations of particular interest or novelty.

Chap. V. *General System.*—Dr. Hall observes, that the principal sources of irritation are the presence of indigestible substances in the stomach, and of irritating matters acting on the mucous membrane of the intestinal canal. The effects of such irritants are either gradual or sudden. The *sudden* effects of intestinal irritation are: acute pain of the head, side, loins, iliac region, or of some other parts of the abdomen; vertigo, dyspnœa, palpitation, vomiting, hiccough; great anxiety and distress, clammy sweat, and sometimes excessive febrile reaction of the heart and arteries.

The effects of exhaustion, when attended with intestinal irritation, evince themselves particularly in the disordered functions of the brain, heart, thoracic and abdominal viscera, and the muscular system.

"The symptoms which affect the head are severe pain, beating and throbbing, rushing or cracking noises, vertigo, or turning round of the room, especially on raising the head or assuming the erect position, intolerance of light and of sound, wakefulness, palpitation, feeling of sinking, of impending dissolution, and being overcome by noise, disturbance, or even thinking, and delirium. The heart is, in different cases, affected with palpitation, fluttering, irregular and feeble action; there are beating and throbbing of the carotids, and sometimes even to the abdominal aorta, a frequent, bounding, and sometimes irregular *pulse*, faintishness, or fainting, urgent demand of the smelling-bottle, fresh air, fanning, bathing of the temples, feeling of impending dissolution, incapability of bearing the erect position, and some-

times early fainting from the use of the lancet. The *respiration* is affected in different cases, with panting, hurry, sighing, great heaving, gasping, blowing, moaning, catching, &c. and, as has been stated, with urgent demand for fresh air. There is, in some cases, an *irritative cough*, in violent fits—or in the form of continual hacking; this cough appears to originate in the larynx or trachea. The *stomach* is liable to become affected with irritability, sickness, retching, vomiting, hiccough, and eructation; the *bowels* with constipation or diarrhœa, pain, flatus, distention, &c. There are very frequently urgent restlessness, tossing about, and jactitation. In some cases, various *spasmodic* affections have occurred. We have often to combat the effects of exhaustion in the *puerperal state*, and in cases in which blood-letting has been improperly employed for diseases not inflammatory, or too lavishly in cases of inflammation."

Our author has noticed a great similarity between the symptoms of the erethismus mercurialis of Pearson, and the effects of exhaustion. Exhaustion or sinking manifests different appearances, according to the different circumstances under which it occurs. Sometimes sinking comes on as a gradual and simple weakness, decline and cessation of the circulatory and respiratory functions; at other times it occurs under the more prominent shape of great inquietude, jactitation, dispnœa, hiccough, &c. Sometimes it has the remarkable effect of dissolving the chain of morbid actions and sensations, constituting the disease under which the patient has laboured, and of presenting to his friends, and perhaps to his unwary physician, the appearance of amendment, when life is soon to terminate in an unexpected dissolution." Lastly, sinking supervenes speedily on the accession of gangrene. According to the observations of our author, there is an extraordinary similarity in the phenomena attending intestinal irritation, exhaustion from hæmorrhage, erethismus mercurialis, the deleterious effects of digitalis, sinking, and dissolution.

Chap. VII. *Respiration.*—In this chapter, the various morbid phenomena exhibited in disease by the respiratory organs are accurately and interestingly described. The following observations on the effects of perfect inspiration and expiration deserve particular attention:

"A deep inspiration is apt to induce cough, when the structure of the lungs is affected with inflammation or disease. But this morbid state is more distinctly ascertained by an attention to the effects of a *full expiration.* In many cases of morbid affection of the lungs, indeed, in which deep inspiration induces neither cough nor other inconvenience, a full expiration not only occasions cough, but other effects, which vary according to the *nature* of the pulmonary disease—in *inflammation of the bronchia* or of *the lungs*, in tuberculous phthisis, &c cough and rattling or crepitus are induced; these effects are also induced in some chronic affections of the lungs, arising either from slight but protracted inflammation, or from disorder of the digestive organs; in cases of *asthma*, too, in which the ordinary breathing, or even a deep inspiration, is unattended with any peculiarity, a full expiration excites both cough and the wheezing sound so characteristic of this affection. It is particularly useful to watch the efforts of a full expiration in the slighter affections of the pulmonary structure, and in the decline of pulmonary disease. Of these two modes of ascertaining disease of the lungs, I have long considered the deep expiration the most valuable, but the most neglected; I therefore strongly recommend it to the attention of the clinical student!"

We conclude this short analysis of Dr. Hall's book, with an earnest wish that this valuable work may soon be republished, and that it may obtain an extensive circulation among the members of the medical profession on this side the Atlantic.

ART. XI.—*An Exposition of the Principles of Pathology, and of the Treatment of Diseases.* By DANIEL PRING, M. D. &c. 8vo, pp. 510. London 1823.

THE subject of the present work is always an interesting one to physicians. The causes of disease, consequently their most appropriate remedies, are still desiderata in many instances. In the investigation of them, we are obliged to delve amidst a vast heap of the rubbish of hypothesis, and overturn the pretensions of many assertions which claim the respect due to established facts, while they are only the result of deceptive experiment.

Our author has reviewed the doctrines of several of the most eminent pathologists of former days; he has, however, neglected others who are at least worthy of consideration; among these we may mention Hoffman and Darwin.

Dr. P. very justly remarks, that "in the theoretical sciences it often happens that the career of improvement is marked by the substitution of a new error in the place of an old one." Cullen's doctrine of spasm, and nervous irritation in fever, is alluded to as an example of this kind. But we cannot see that our author has offered any other doctrine calculated to take the place of that which he wishes to overturn.

He observes that a sudden suppression of perspiration is generally considered as a cause of fever. He thinks, however, that the correctness of this doctrine may be doubted, when we consider the numerous instances in which the perspiration is suddenly checked without any ill consequence. Those men, for example, whose occupation exposes them to an atmosphere of high temperature, with frequent transitions to a cooler air, such as glass-blowers, bakers, &c. ought, according to Cullen's doctrine, to be continually liable to fever. But the force of habit, in these instances, overcomes the evil tendency of these exposures. On the other hand, who has not seen colds, rheumatism, pleurisy, and various forms of pyrexia consequent to sudden transition from a warmer to a

colder medium, when the body was covered with perspiration? The causes of disease, like other natural causes, are controlled by circumstances: and there are some constitutions which can bear with impunity those exposures which would be fatal to others.

In some instances there appears to be a constitutional predisposition, or hereditary tendency to a certain disease, which, under these circumstances, will require but a slight degree of an exciting cause for its development. We may mention, as examples, gout, rheumatism, hepatitis, and phthisis. There are others again, which being free from any morbid predisposition, preserve their balance against ruder assaults. But we have great faith, also, in the power of habit. The sailor who is continually exposed to heat and cold, to sunshine and rain, feels no inconvenience from an exposure which, under other circumstances, would fully illustrate the doctrine of Cullen.

Our author admits, that in case of suppressed perspiration, "the entrance of fluids into the extensive system of cutaneous vessels is resisted by a preternatural contraction of these vessels," it is then "obvious that those vessels which are not cutaneous, must contain more than their natural proportion of fluids; this state would be one of general internal plethora. But we observe that, as a consequence of suppressed perspiration, one organ always suffers principally; that there is a particular determination to one seat. If a contracted state of the cutaneous vessels could throw an unnatural proportion of fluids into the internal ones, so preternatural dilitation of the vessels of any other seat or system would deprive the cutaneous vessels of their natural proportion. We have then to choose whether disease is maintained by suppressed perspiration, or whether the suppression of perspiration is a consequence of disease."

This reasoning may be ingenious, but it assumes a latitude of probability, and a liberty of transposition of cause and effect, which, if generally adopted, would render all rea-

soning vain. We continue our quotation:—"If we consider here *merely the order of occurrence*, it would appear that the disease, say, for example, acute rheumatism, was produced by suppressed perspiration." Hence it appears, that the order of occurrence affords no certain ground of distinction between cause and effect! We fear that this will prove a very inefficient method of getting rid of the practice of "substituting a new error in the place of an old one," for the improvement of pathology.

With regard to the circumstance, that in consequence of suppressed perspiration one organ is found to suffer rather than others, our author observes, that "it is necessary to suppose a predisposition in such seat (or organ) which does not exist in others. This predisposition is nearly allied to disease; and the admission of it goes near to confessing that the circumstance of a suppressed perspiration was preceded by processes of disease in that seat which afterward exhibits its chief phenomena."

It is, indeed, very difficult, and in some instances impossible, to explain *ad demonstrationem*, why one particular organ should be the sole seat of a disease, the operation of whose cause is referred to general principles. But we think an organ may be peculiarly liable to a morbid impression, through mere debility, or from other causes entirely unconnected with the morbific agent. Dr. Pring must attack the fortress of Cullen by some heavier artillery, ere he effects a practicable breach.

With regard to the Brunonian system, our author has given the slain Percy another wound. In other words, he has laboured to demolish those parts of the system of Brown which have long since been thrown aside among the rubbish of forgotten things. He has, however, given this celebrated author credit for one judicious practical maxim, namely, the propriety of repleting very slowly after extensive depletion.

"In the beginning of convalescence," says Dr. Pring, "I have known a single meal, of a nutritious or stimulating quali-

ty, to which the system had long been unaccustomed, under the previous treatment of abstinence and depletion, followed by apoplexy, which was fatal in a few hours; and I have known the first stimulating meal taken, contrary to direction, twelve days after a delivery, which was attended by profuse flooding, followed immediately by accelerated pulse, pain in the head, eventually coma and death. In a similar case, a glass of porter, given injudiciously four days after delivery, has been immediately followed by apoplexy."

Our author does not approve of the practice, common in such cases, of drawing blood "copiously and frequently while the symptoms continue." He does not appear, however, to object to a moderate use of the lancet. His reliance is almost entirely on purgatives. "I have been in the habit of confiding in purgatives, to the almost total neglect of the lancet; but these purgatives have not been of a milk and water kind. If, after copious loss of blood, the pulse has suddenly *got up* from eighty to a hundred and thirty or forty in a minute, with intense pain in the head, perhaps slight incoherence of speech, restlessness &c., I have given six grains of calomel, and the same quantity of James' powder, followed by a two-ounce draught of salts and senna, with half a drachm of jalap in it. If this has been rejected, it has been repeated in less than an hour, and repeated as often as it was rejected, until it has produced copious evacuations from the bowels. One, and *perhaps** an important effect of such purgatives, is to make a great revolution in the whole intestinal canal, which is commonly followed by almost perfect relief of the head, and, *perhaps*,* an immediate subsidence of the pulse to a hundred, or a hundred and ten. The tendency to disorder of the head is afterward easily kept within safe bounds, by small but repeated doses of Epsom salts."

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* Why does our author guard himself by the adverb *perhaps*, in these instances? It is the primary action of a cathartic to *make a revolution*, as our author expresses it, in the alimentary canal—and a reduction of the pulse is a natural and never failing consequence of active catharsis.

Dr. P. is of opinion that putting the system suddenly under the full influence of mercury, is the best method of subverting the disposition to cerebral disorder. But, whatever the Dr.'s experience may have been, we cannot consent to relinquish the lancet in cases of cerebral plethora or engorgement. Many cases occur, and perhaps they are the most numerous, in which the life of the patient depends upon instant depletion, and where it is impracticable to administer medicine by the mouth—even the delay of administering an *enema* would be dangerous, to say nothing of the inconveniences, and, often, impossibility, of using the œsophagus catheter. Again, cases of cerebral determination sometimes occur during convalescence from excessive mercurial impression;*—would Dr. P. recommend mercury in such cases?

On the subject of visceral congestion, our author expresses an opinion, which, by the way, if not involving a self-evident truth, at least reiterates a long established maxim. "I apprehend," he says, "it is generally, though not, perhaps, invariably true, that if visceral inflammation or congestion in fever can be kept within safe bounds, very little is to be feared from the constitutional state; and that the disease will, almost without an exception, run a safe, though a protracted course."

Our author's opinion, that stimuli are only useful in the advanced stages of typhus, is certainly correct, and it has been the rule of practice with all intelligent practitioners from time immemorial. We have witnessed the fatal effects arising from the exhibition of stimuli, in the earlier stages of typhus, by timid practitioners who feared the state of debility necessarily consequent, as they thought, to a free evacuation of the intestinal canal. We believe with Dr. Pring, that many cases of typhus gravior have been the result of improper prac-

* A case of this kind fell under our notice a few years ago. A young man, during convalescence from a bilious fever, for which he had been profusely salivated, suffered, almost daily, from a sense of weight in the head, temporary loss of memory, and disposition to sleep.

tice in the early stage of ordinary typhus, and might have been prevented by the timely use of the lancet and free catharsis. Dr. P. does not appear to have much faith in stimuli in any stage of typhus. The mercurial action is his chief dependance and last resort.

In extreme cases he recommends ten grains of calomel to be given every six or eight hours, until ptyalysm be produced, when all the threatning symptoms speedily disappear. We have no objection to this practice; on the contrary, we have so frequently witnessed the excellent effects of the mercurial action in typhus, and in every form of bilious disease, that we would not willingly undertake the management of a case without the privilege of resorting to this remedy. But we think that our author defers its application too long—he thinks that after coma has supervened, dependance may be placed in this remedy. We would rather exhibit calomel, in smaller doses, immediately after the intestinal canal has been well evacuated by powerful cathartics.

In our author's chapter on what Hoffman calls the *pathology of the blood*, we see nothing new, except his assurance that he has paid *considerable attention* to the question, whether the *arteries of an inflamed part beat more rapidly than those of a healthy part, of the same subject!* It is not necessary to state the result of this *considerable attention.*

The imposing title of the present work, urged it on our notice. We have thought proper to give a slight sketch of its character, though our limits confined us to a very few pages. But this is a circumstance not to be regretted:—we could not extend our review advantageously to our readers, as the book contains nothing new that may be useful, and no errors that have not been repeatedly exposed.

T. M. H.

ART. XII.—*An Essay on Venereal Diseases, and the uses and abuses of Mercury in their Treatment; illustrated by drawings of the different forms of Venereal Eruptions.* Second Edition. By RICHARD CARMICHAEL, M R. I. A. Vice-president of the Royal College of Surgeons in Ireland, &c. &c. Octavo, pp. 376. London. 1825.

As our author observes in his advertisement, public opinion has been materially altered since the first edition of this work appeared, and "instead of the belief that no venereal complaint can be cured without mercury, it is now very generally acknowledged that every form of venereal disease may be successfully treated without that remedy." But he admits that practitioners are at present quite unsettled in their opinions respecting the best mode of treating these complaints, and that probably no two think alike on the subject. He thinks, however, that "even in the present state of uncertainty much good has arisen; for we seldom now witness a constitution run down by the excessive use of mercury; or find that *that* is now ever exhibited, when it is obvious that a consumptive tendency, or other causes, might render it hazardous to the patient's constitution." He asserts it as a fact, that the consumption of mercury has been materially lessened within the last ten years, which have succeeded to the publication of his first edition. Many apothecaries in Dublin have informed him that there is not a sixth of the quantity consumed which was formerly used in that metropolis, and he has not a doubt that even this proportion will continue to meet with a still further reduction.

He does not think, however, that the use of mercury ought to be entirely discarded in the treatment of venereal complaints; on the contrary, he believes that, when considerately applied, it will be found a most useful auxiliary; but it is merely as an auxiliary, and not as an indispensible specific, that he would wish to have it considered. These ideas are so correct, and correspond so exactly with the good sense of

every reader, that they cannot fail of ensuring a most respectful degree of attention to every opinion which Mr. Carmichael may have had occasion to advance in connexion with them. We should feel justified, therefore, in occupying our pages quite extensively with an analysis of this interesting and valuable work, were it not for the fact that the former edition has already been placed in the hands of almost every student throughout the country. On the present occasion it will only be necessary to point out to the attention of our readers such parts as appear to be either improvements or additions to the former publication of our author.

Besides the yaws, the sivvens, and the Canadian disease, he now describes two other distempers which bear no remote resemblance to the succession of constitutional symptoms in syphilis. The first of these is called the lepra septentrionalis, by Callisen, and occurs in the maritime parts of Norway, Sweden, and Russia. It is found, also, on the shores of Iceland, in the Feroe islands, and in some parts of Scotland. It is called the *radesyge*, by the Norwegians; and is placed by Dr. Holst, an author of the same country, between the exanthemata and cachexiae. It commences with lassitude, anorexia, rheumatic pains, rigidity of the limbs, dyspnœa, and violent head-ache. The face either assumes a singular shining silvery appearance, or a deep red or livid colour. These symptoms may continue for many months before the horrible condition, to which they are the prelude, ensues. A dry branny efflorescence first takes place, which renders the skin rough and thickened. Soon after, copper-coloured tubercles form on the skin, and herpetic spots on the forehead, nose, chin, lips, and eye-brows; afterwards these become scattered over every part of the body.

"The integuments of the forehead become thickened and corrugated, the eye-lids swell and are everted, the bloated cheeks assume a livid colour, in fact, the entire countenance becomes frightfully distorted. The spots afterwards degenerate into malignant ulcers, which even extend to the bones

underneath, and render them carious. Every part of the fauces is also liable to be affected with similar tubercles and ulcers which extend to the nares. The bones of the palate and nose soon become carious. Frightful deformity ensues; and the patient, exhausted by so great an extent of disease, dies of hectic fever."

This disease is not considered by Dr. Holst, who published an excellent account of it in 1817, as arising from sexual intercourse; it is attributed by him and by all others who have had an opportunity of examining it, to the mode of life followed by the northern Swedes and Norwegians, but more particularly by the fishermen inhabiting the coasts of those high latitudes. They are extremely filthy in their persons, they breathe a noxious and offensive atmosphere in small and ill ventilated huts, and they live upon the most rancid and indigestible kinds of food.

Of course, the best mode of averting this terrible distemper consists in free ventilation, cleanliness, bathing, and in the cultivation and use of esculent vegetables. Callisen thinks it may be cured in the first stage by the use of baths, and a milk and vegetable diet, with fresh animal food. Among the medicines which he recommends, are cicuta, antimonials, rosemary, &c. He thinks that mercury, when carried to the extent of salivation, is always injurious.

The second disease above alluded to, is, according to our author, peculiar to his own country, and is called by the natives *the button scurvy*. It has not been described either by Willan or Bateman, or by any other writer within his knowledge, although it is frequently met with in the Dublin hospitals. It occurs only among the lowest class of peasantry engaged in agriculture.

"The spots of this eruption are convex *tubercles*, varying from the size of a split pea to that of a shilling, seldom so large as half-a-crown; they exhibit an appearance somewhat like the surface of a raspberry, and discharge a white tenacious matter, which forms thin yellow crusts that are in general

rubbed off by the patient's clothes. The spots are seldom seen on the face, but are scattered over the trunk and extremities, and are almost always observable on the inside of the thighs, or close to the axillæ."

The general health does not appear to be much disturbed in this disease, which may continue for months and even years in the same individual. It is generally cured by the use of the warm bath, and the exhibition of pitch pills in as large doses as the stomach can bear, in the course of six or seven weeks. Our author does not think it to be infectious, but attributes it to the smoky, unventilated, and filthy habitations of the lower classes in Ireland. He thinks it may be confounded with the eruption which succeeds the phagedenic venereal disease, unless it is pointed out to the notice of practitioners.

After having described such diseases as appear to resemble the constitutional forms of syphilis, our author next proceeds to the detail and classification of those which begin by primary affections of the genital organs, and terminate in secondary contaminations of the general system. This is by far the most important part of his work,—inasmuch as until the first publication of it, practitioners had almost universally continued the indiscriminate use of mercury in every variety of genital diseases, notwithstanding the observations of Hunter, of Adams, and of Abernethy, in regard to the impropriety of such a practice. We do not know to what extent the classification of our author, as published in his first edition, has been adopted by the profession; but from the bold and decisive tone with which he now speaks of the correctness of his opinions, as tested by long and reiterated experience, we are disposed to regard it as much better established than we had before imagined. He insists upon it over and over again, that the different species of secondary eruptions always bear a determinate relation to the primary symptoms from which they originate; and he of course thinks himself justified in submitting them to the strictest rules of nosological arrangement,

In this edition he has changed the order of his classification somewhat, inasmuch as he has placed the true syphilis of John Hunter, which he supposes to have been imported into Europe by Columbus, in the last department of venereal diseases. As he has given, in the conclusion of this edition, a most excellent synopsis of his four different species, with the remedies appropriate to every form and stage of them, we will insert it without any abbreviation, instead of attempting to present an analysis of the body of the work.

I.

PAPULAR VENEREAL DISEASE.

Primary Symptoms.	*Remedies.*
1. The simple primary ulcer, 2. Patchy excoriation, attended with a discharge,	Astringent washes. Antimonials. Purgatives.
3. Gonorrhœa virulenta,	Astringent washes. Antimonials. Terebinthinates.
4. Buboes.	Leeches. Cold applications. Blisters. Poultices.

Secondary Symptoms.	
1. Papular eruption, preceded by fever, and ending in desquamation,	General blood-letting to be adopted when indicated by the fever, and proportioned to its degree. Antimonials.
2. Ærithematous inflammations of the fauces.	Sarsaparilla.
3. Swelling of the tonsils and glands of the neck.	Mercury unnecessary in any stage, and highly injurious until the eruption desquamates, the fever is subdued, and the disorder is evidently on the wane; and then an alterative course of antimony and calomel may occasionally accellerate the cure.
4. Pains in the larger joints, resembling rheumatism,	
5. Iritis,	*Full mercurial affection of the system, until the inflammation is subdued.* Local bleeding. Blistering.

II.

PUSTULAR VENEREAL DISEASE.

Primary Symptoms.	*Remedies.*
1. The ulcer with elevated edges, without induration,	Astringent washes. Antimonials. Purgatives.
2. Buboes,	The same treatment as for the buboes in No. I. Excision of the undermined edges of the buboe.

Secondary Symptoms.	
1. Eruption of pustules, in general phlyzacious, preceded by fever and terminating in ulcers, covered with thin crusts that heal from their margins, and when the disease is on the wane, the eruption desquamates into scaly red blotches.	General blood-letting, as in No. I. Antimonials. Sarsaparilla. Guaiacum. Tar ointment. Baths of sulphuretted kali. Sulphurous fumigations. Nitro-muriate acid baths. *Mercury is decidedly pernicious until the pustules terminate in scaly blotches instead of forming ulcers, and then mercurials, in alterative doses, conjoined with sarsaparilla or guaiacum, may occasionally be employed with benefit.*
2. Ulcers on different parts of the fauces, in general of a white aphthous appearance,	The same general treatment, conjoined with the following local applications: Common detergent gargles. Mercurial gargles. Oxymel Æruginis. Fumigations of Hydrarg. Sulphuret. rub. or of Hydrarg. cum creta.
3. Pains in the joints,	The same general treatment conjoined with the following local applications: Leeches. Fomentations. Bread and water poultices. Blisters. Ointment of tartarized antimony. *Mercury is to be particularly avoided while inflammation of the knee exists.*
4. Nodes,	The same general treatment conjoined with the following local applications: Leeches. Bread and water poultices. Blisters. Ointment of tartarized antimony. Division of the periosteum. *If the preceding remedies prove inefficient, mercury may be of advantage, provided the general disease is on the wane.*

III.

PHAGEDENIC VENEREAL DISEASE.

Primary Symptoms.	*Remedies.*
1. The phagedenic ulcer,	Bread and water poultices with opium occasionally added. Warm fomentations. General blood-letting, proportioned to the acuteness of pain, inflammation and symptomatic fever.
2. Sloughing ulcers,	Antimonials in nauseating doses. Opium, Cicuta, Hyoscyamus } in full doses. Purgatives.
3. Buboes,	The same treatment as for buboes in No. II.

Secondary Symptoms.	
1. Eruption of tubercles, or spots of a pustular tendency, or both intermixed, and preceded by fever, and terminating in ulcers covered with thick crusts; which often assume a conical form, healing from their centre, and extending with a phagedenic margin. When the disease is on the wane, they desquamate like those of No. II. into scaly blotches. The disease is attended with great debility, particularly when the ulcers are extensive,	General blood-letting as in No. I and II. Antimonial. Sarsaparilla. Guaiacum. Compound powder of ipecacuanha. Opium. Cicuta. Nitrous acid. Nitro-muriatic acid bath. *Mercury (with the exceptions noticed below,) increases the ravages of the disease in all its stages but the last. It may then be given with safety and advantage as in No. II.*
2. Ulcers on different parts of the fauces, and particularly the back of the pharynx—frequently engaging the entire fauces, and sometimes extending to the larynx and narex,	The same general treatment conjoined with the following local applications: Oxymel æruginis. Strong solution of muriate of mercury, or nitrate of silver. Fumigations of Hydrarg. Sulpuret. rub. or of Hydrarg. cum creta. *If these should prove inefficient, mercury may be used largely with advantage in checking the progress of the ulcerations, even though it should exasperate the general disease.*
3. Ulcerations and thickening of the larynx,	Caustic issues, or applications for moxa on each side of the thyroid cartilage. Tracheotomy.

4. Ulceration and caries of the bones of the nose,	The same general treatment with the yellow and black mercurial washes frequently injected into the nostrils. Fumigations of the Hydrarg. sulphuret. rub. or of Hydrarg. cum creta through the nares. *If these remedies fail, mercury may be used as above.*
5. Pains of the joints,	The same treatment as in No. II. for the same symptom.
6. Nodes,	The same treatment as in No. II.

IV.

SCALY VENEREAL DISEASE.

Primary Symptoms.	*Remedies.*
1. The chancre, or callous ulcer, 2. Buboes,	*Full courses of mercury for both primary and constitutional symptoms, except a tendency to phthisis pulmonalis, or other delicacy of constitution should forbid them.*

Secondary Symptoms.	
1. Eruption of scaly blotches presenting either the character of lepra, or psoriasis, and unattended with any obvious degree of fever,	*Mercury.*
2. Excavated ulcers of the tonsils.	
3. Pains in the joints, tibiæ, cranium, &c.	The same local remedies as prescribed for the several symptoms in No. II.
4. Nodes.	

Our present limits will not allow us to give this volume a much longer analysis; indeed, until the above extracted synopsis had been put in type, we did not discover that the edition now under notice differed very essentially from the former one, which Mr. Carmichael published about ten years ago. On examining it a little more leisurely, however, we are gratified to find that it is filled with much new and interesting matter, and that, unlike most editions of republished works, it is so carefully remodelled and altered throughout, as to become almost a different kind of publication. The details of numerous cases are, we think, judiciously omitted to make room for practical and theoretical discussions, and the original size of the book has been enlarged by some elaborate, and apparently satisfactory replies to the various objections

which have been urged against his doctrines. On account of all these circumstances it appears to us that a republication of the present edition would be of great service to the profession in this country; and it will afford us much pleasure to find that we have been, in any measure, instrumental in bringing about so important a transaction.

Several anonymous attacks have been made in the periodical publications of Great Britain, against the doctrines advanced in this work; but no one of them deserves a serious consideration from our hands, inasmuch as they have not been deemed worthy of attention by our candid and dispassionate author. The objections of Mr. Guthrie, however, were urged in so fair and manly a manner, that they have drawn forth a long and interesting reply from Mr. Carmichael. The chief point in dispute between these truly eminent surgeons lies in the question with regard to the specific nature of the primary and secondary symptoms of phagedenic ulcers. Mr. Guthrie thinks that common ulcers become phagedenic by ill treatment, and afterwards produce the same constitutional symptoms which are attributed in this work to the specific phagedenic poison. He believes, also, that indolent ulcerations may produce the same secondary eruptions as usually follow the real phagedena; and that phagedenic and sloughing sores may exist without being succeeded by any specific kind of affection. Mr. Carmichael, however, shows in a very satisfactory manner that his opponent may have been led to form false conclusions from the imperfect and hasty observations which he happens to have made upon the nature of the various kinds of ulcers which fell under his notice. Many real phagedenic sores are very slow in their progress, so that frequently they can only be distinguished from common ulcerations by the slightly undermined edges with which they are surrounded. Such might easily have been mistaken for indolent ulcers, and this, with other like mistakes, which are fully pointed out by our author, may have furnished Mr. Guthrie with the most weighty objections which he has advanced against the new classification. M.

COMMUNICATION.

A Brief Inquiry into the Merits of the Sulphate of Quinine. By Thomas D. Mitchel, M. D. of Frankford.

The design of this essay is to exhibit to the medical profession, what is believed to be the actual character of the sulphate of quinine, as a remedial agent. A correct view of this subject is deemed to be of great importance, inasmuch as some practitioners were led to adopt and, ultimately, to reject the medicine, from the influence of example only, without pausing to examine, compare and judge for themselves. This has been the case with many other articles of materia medica; and it is probable that good medicines have been discarded, and others, altogether worthless, made popular, by virtue of a similar inattention.

Is the sulphate of quinine a valuable remedy? Is it preferable to many articles that are employed with similar designs? If these questions be susceptible of an affirmative reply, it becomes us, as judicious and honest practitioners, to yield it the meed of praise to which it is entitled. But, say some, "it is only another form of our ancient and invaluable remedy, the bark, and to give it a preference merely because it comes to us in a new shape, serves no other end than to gratify that love of novelty, which is so injurious to the profession." This kind of reasoning, however, (if by that name it may be called,) is not entitled to any weight in the present case, and could be applied with equal force to the rejection of some of the best articles in every-day practice.

It will be understood that the writer, in attempting to set forth the actual merits of the quinine, speaks for himself. and therefore presumes not to give the opinions of others. He has employed the article to a very considerable extent, and believes that his opportunities of testing its real value, have

been amply sufficient to enable him to arrive at a just conclusion. Nevertheless, let every man judge for himself.

The sulphate of quinine came into use in this region of country, in the first instance, as a remedy for the intermittents which so universally prevailed. Not that the disease refused to yield to the remedies of which we were already possessed, but because the first trials of the article in France, warranted the belief that it greatly accelerated the patients' cure. The bark had not lost its efficacy to interrupt the morbid associations by which the periodical attacks were perpetuated, but in very many instances it ceased to be remedial, because its administration was impracticable. In hundreds of cases, the irritability of the stomach was so great, or so powerful an idiosyncrasy reigned in the system, that it could not be taken in any dose, however small. Nor was that Herculean medicine, the arsenic, shorn of all its honours, by the introduction of the quinine. It was eminently successful, and had many advantages over the bark, the chief of which was, the facility with which it could be administered. But there was, notwithstanding its efficacy, a popular dislike respecting it, which was increased by the well known fact, that its use was often followed by œdematous swellings.

It is not matter of admiration, that when all the ordinary remedies for intermittents were attended by so many difficulties, the annunciation of so auspicious a medicine as the sulphate, and under circumstances so authoritative, should have been received with such general approbation. That some were extravagant in their encomiums, cannot be denied, and it will be readily conceded, that with a few it was likely to become an idolized panacea. But the illusions peculiar to novelty, however influential they may have been, in relation to the sulphate, have long since passed away, and we may now form our opinions, from the sober dictates of experience and observation.

In the common fever and ague, there is almost always so much gastric and intestinal derangement, as to render emetics

or cathartics the proper precursors of medicines intended to prevent the recurrence of the cold stage. This remark is strictly applicable to the use of the sulphate of quinine, which, so far as my experience goes, seldom failed to aggravate the subsequent attack, unless preceded by evacuants. In the common quotidians, it is rarely practicable to administer a sufficient quantity of bark during the intermission, to produce any good effect; and although the arsenic may be given without any reference to intermissions, yet from three to five days are generally requisite to effect a cure. You may, it is true, give enough bark in some quotidians, to effect a postponement of the next attack, and so, gradually to overcome the disease. But these difficulties do not meet us in the use of the quinine. A patient is attacked in the morning, at eight or nine o'clock, with a violent chill; by ten it has subsided, and the hot stage has supervened, with all its concomitants. If the proper means are not employed, the febrile stage will continue, perhaps till eight or ten in the evening, and at six of the next morning, the cold stage will return with increased force. The plan to be pursued, is to shorten the febrile stage, and so to induce the sweating stage. This is best effected by an active emetico—cathartic of calomel, ten grains, and ipecacuanha, fifteen grains, followed by a table-spoonful of spt. mindereri every fifteen minutes, together with the application of bottles filled with hot water to the surface, and sinapisms to the extremities. Rarely have I found bleeding to be necessary, but have been enabled by the above plan, to induce a proper state of the system in a few hours, to warrant the exhibition of the quinine in the following manner, *viz:*—℞ Sulph. quin. gr. x.—xii. Pulv. Caryophyll. gr. iv.—vi. m. ft. pulv. iv.—one to be taken every half hour. Thus I have generally succeeded in arresting the disease; and in order to guard against a return of it, have prescribed a grain of the sulphate, to be taken every day for several weeks afterwards. This rule should be strictly complied with.

I have seen cases of double quotidian, which, I am certain, could never have been cured by the bark, but which yielded readily to the sulphate of quinine. In one of these cases, the chill came on early in the morning, followed by the febrile stage, which continued till three or four in the afternoon, when the patient was seized with a second chill. Here it was impossible, by the most active treatment, to procure an intermission of more than one hour, and the mere mention of the bark excited nausea. In that case, eight grains of the sulphate of quinine were given in less than an hour, by which the expected chill was reduced to a trifling sensation of coldness, followed by so little of fever, as to enable the patient, previously to the hour of bed-time, to take eight grains more of the medicine. Her cure was effected, ultimately, by the daily use of one grain of the article.

The bark is not only often rejected by the stomach, but it sometimes occasions a looseness of the bowels, by which the patient is debilitated, without having any check put to his periodical attacks. This, to be sure, can be corrected by the exhibition of opium, but it is a fault from which the sulphate of quinine is exempt. It is my opinion, decidedly, that no medicine will so soon and so certainly prevent a recurrence of the cold stage as the quinine. Evacuate the stomach and intestines, and effect an intermission, and you may then exhibit the sulphate of quinine with entire confidence in the result. It agrees with the stomach much better than any other tonic with which I am acquainted.

But, however successful the sulphate of quinine may have been in the cure of ague and fever, it has appeared to me to be much more triumphant in the bilious remittents which we have had so frequently to encounter. The old methods of treating remittents, generally protracted their cure to five or six weeks. Indeed, it was common with some practitioners to affirm that it was impossible, by any means, to prevent those fevers from running their course; and it appeared to me, that, according

to their scheme, the bark answered no other end than to keep up the energies of the system, while nature conducted the patient through, what was supposed to be, the unalterable course of his disease. The sulphate of quinine has taught me, (if I had not learnt before) the fallacy of such pathological views; and from the experience I have already had, I feel as confident of being able to cut short the course of a remittent, as I am that I can relieve the pain of tooth-ache by extracting the offending cause.

It were an unnecessary waste of time to detail cases illustrative of the truth of my assertions. Suffice it to say, that I have repeatedly reduced the attendance of such cases of remitting fever, as formerly required weeks for their cure, to almost as few days, and that, mainly, by the agency of the sulphate of quinine. Nor is this the only advantage, since by a bold and timely use of this article, I have avoided the dangerous mistake of superinducing a tedious and often fatal stage of disease, which, if it be not typhus, bears a considerable resemblance to it.

The common method of treating remittents has been, to administer a cathartic or an emetic in the first instance, to follow this with some sudorific, and so soon as a remission ensued, to exhibit the decoction of bark and snake root. The latter being inadequate to subdue the tendency to a return of fever, seemed to do nothing more than act as a partial preservative of the system against the violence of febrile action. Hence it was given for days and weeks, without effecting any obvious end, until at length, to use the fashionable medical parlance, the disease ran its course, or wore itself out. The arsenic has, indeed, been more efficient in the hands of some practitioners, but it is quite inferior to the sulphate of quinine. In employing the latter article in remittents, I made it a principal object to induce a remission as speedily as possible, and then to exhibit the sulphate of quinine with so much vigour as to prevent a repetition of fever. Finding my patients,

uniformly, more or less affected with gastric and intestinal derangement, I gave, immediately, a dose, of calomel, six grains, and ipecacuanha, ten grains; to be repeated in two hours, unless the first portion acted powerfully. This was usually followed by an ounce of epsom salts, accompanied with a tablespoonful of spt. mindereri, every quarter or half hour, together with sinapisms and the like. If the head was much affected, ice applied in bladders generally gave immediate relief. Under this method of treatment, I could, for the most part, induce a complete remission within the first three days; and then, at least ten grains of the sulphate were administered in the course of two hours, agreeably to a prescription given above. This was my ordinary plan, and where no unforseen circumstance occurred to prevent its fair application, it always produced the desired effect. The febrile attack, so much dreaded, did not recur, and the decoction of bark, or small doses of the sulphate of quinine, with occasional evacuants, were fully adequate to the restoration of the patient in a week or ten days. I am now entirely satisfied, that no practitioner can, without criminal neglect, suffer an ordinary remittent to run its course, and so confine the patient to his bed four, five, or six weeks. He need no longer be the obsequious servant of nature, but may control her at his pleasure.

As a tonic in simple debility, I have been much pleased with the sulphate of quinine, and have found it greatly serviceable in cases where I could not use any other preparation of the bark.

The modes of administering the sulphate of quinine are various. For a long time I preferred the pill, and generally found that patients took it more readily than any other preparation. Small children, however, take the liquid dose best; I mean the solution of the sulphate in mint or cinnamon water, with sugar and gum arabic; and perhaps this is the best preparation that can be given in such cases. In several instances, I found it impracticable to administer the quinine un-

less I resorted to a species of deception which I believe to be perfectly warrantable in like circumstances. A prejudice had obtained against the "pills," as the medicine was commonly called, and patients would not take them. It was necessary to give assurances that the disease could be cured without resorting to the "pills," and then I prescribed the powder in combination with cloves or ginger. For an adult, the following was the ordinary recipe:—Sulph. of quin. gr. x. —P. caryophyll. gr. vi.—m. ft. pulv. gr. iv.—one to be taken every half hour. This plan proved to be the best I could employ, since I was able to give a large dose in a shorter period of time, than by any other mode. Taken in syrup of any kind, these powders have a taste differing but little from the common tonic powders, and are swallowed without objection. In remittents, this is decidedly the best way of giving the sulphate of quinine. There is another mode of administration with which I have been highly pleased, especially in the cases of aged persons. It is as follows, viz:—℞ Sulph. quin. gr. vi.—x.—Tinct. C. Peruv. Hux. ℥i, elix. vitroil, ʒi, aquæ pur. ℥ij—m. Take a tablespoonful every two hours.

The impure sulphate of quinine, known by the name of the *brown extract*, obtained some reputation in the treatment of intermittents; but I am sorry to be obliged to dissent from several of my medical brethren respecting its claims. I gave it frequent trials, but was compelled to lay it aside, because, on the whole, it proved to be much inferior to the pure sulphate. In many instances, it sickened the patient considerably, and frequently failed to effect a cure. In my hands, the pure sulphate has never occasioned vomiting, and seldom much nausea, where proper evacuants had preceded its use; its success, moreover, has been uniform.

In addition to the modes of using the sulphate of quinine, already stated, I have given it in Lisbon, Madeira, and other wines, and think the preparation an excellent preventive ton-

ic, and as such entitled to a place in every family, exposed to the miasmata, or other sources of ague and fever.

There is another preparation of the sulphate of quinine, which I cannot omit to notice in this place, as being admirably suited to cases of defective tone of the stomach and alimentary canal, attended with occasional spasmodic pains. It must be preceded by proper evacuants: ℞ sulph. quin. gr. viij.—x.—carb. ammon. gum camphor, āā gr. x.—P. G. arab. et sacch. alb āā ʒij aquæ memthæ p. ʒvi.—m. s. a.—Dose, a table-spoonful every hour or two, according to circumstances.

The sulphate constitutes an excellent substitute for colombo or gentian, in the tonic powders, and in this way I have frequently used it. ℞ sulph. quin. grs. x.—rub. ferri ʒi. P. caryophyll. ℈i—m. ft. pulv. x.—One to be taken every three or four hours, in syrup.

Very many objections have been raised against the sulphate of quinine. Of these I shall not condescend to take any notice, believing them to have originated, generally, in motives unworthy of the medical character. Others have been urged with that candor and sincerity that entitles them to respectful consideration. It has been alleged that head-ache has been a more common disease, since the general use of the sulphate, than previously to its introduction. That this may have appeared to be the fact, in the experience of some physicians, I cannot but believe. As respects my own observations, however, I am bound to exonerate the remedy from this charge. I have not met with the smallest increase of cephalic affections since I have exhibited the sulphate of quinine.

Our medicine has also been accused of inducing hemorrhages and convulsions, and at the first view, this charge may appear to be a little plausible. In regions of country where, during the last three years, intermittents have prevailed almost universally, there were many cases of hæmorrhage among adults, and of convulsions in young children, and as the quinine was in general use, these effects were ascribed to

its agency. By a little attention to this allegation, I have ascertained, that in my own practice, the proportion of cases attended with hæmorrhage or convulsions, was quite as large before as after the introduction of this article. I witnessed both these concomitants of the epidemic, while I gave the arsenic alone, prior to the use of the sulphate in this country. And as respects these or any other pernicious effects of the sulphate, my experience is altogether without evidence.

As regards the cost of the sulphate of quinine, whatever clamours may have been raised against it in its early history, there can be no ground of complaint now. It is as cheap a remedy as the best bark at the ordinary price; but when considered in connexion with its more speedy operation, the comparison between it and any other article is decidedly in its favour.

The result of our inquiry seems to be, that although the bark, arsenic, &c., are really good medicines, yet that the sulphate of quinine is much better. That while we admit the curative powers of the former, we have a right to insist upon the superior virtues of the latter; and this, chiefly, because it is a remedy of universal application, or, in other words, because all patients can take it in one form or other, which is true of no other remedy for intermittents or remittents. That the sulphate of quinine may not yet be superseded by a better remedy, we venture not to affirm, because we regard our knowledge of cause and effect, in reference to the cure of disease, as being only in its infantile state. Our duty, however, is to take a stand, and to form our views according to the existing state of things; in so doing, we cannot but subscribe to the superiority of the quinine over every known article, as a remedy for those forms of disease for which it is professedly designed.

ANALECTA.

1. *Nervous Pregnancy.*—Some of our readers, not deeply versed in obstetric literature, may start at seeing the title of this short article. But many of the most celebrated accoucheurs have witnessed or written on this species of pregnancy. Baudelœque says he saw more than twenty cases of it, some of which continued for several years. M. Gerard, of Lyons, has published a memoir on this subject; but the present case, related by M. Russel of Vars, in the first number of the Gazette de Santé for the present year, is, perhaps, the most remarkable on record.

Case.—Mary Gibaud, residing at Vars, department of the Charente, had enjoyed good health previous to her marriage. Shortly after this epoch, she became apparently pregnant. The menses ceased—nausea and morning vomitings occurred—the abdomen enlarged—the motions of the fœtus were felt or supposed to be felt—in short, every symptom of pregnancy was present. At the end of nine months, labour-pains commenced, and went on increasing for thirty-six hours. The midwife, unable to make out the case, called in a surgeon of great reputation. At the moment of his arrival, the patient had just fainted from a considerable uterine hæmorrhage, and the surgeon quickly proceeded to deliver. He was not a little surprised to find the uterus in an unimpregnated state. On recovering from syncope, the labour pains had gone, but in two or three hours they returned as violent as ever. The surgeon now bled her copiously from the arm, and all the unpleasant symptoms vanished. During a month, she continued well—the abdomen still large. At the end of this period, the nausea and vomitings again commenced, and the usual symptoms of pregnancy went on for nine months, when the (apparent) labour pains again recurred, and again ceased, after a natural and artificial loss of blood similar to that stated above. Then a month's interval, and then again symptoms of pregnancy, and so on. This state continued for three or four years, when the case getting abroad, the patient was visited by several professors of different universities. In the fourth year, she was carried to the hospital of Angouleme, where the physicians considered the case as dropsy, and paracentesis abdominis was performed, but without evacuating any fluid. She recovered as usual, and went on for twenty years with all the symptoms of pregnancy, and every nine months a kind of attempt at parturition, to be relieved only by uterine hæmorrhage and a copious bleeding.

For the last five or six years, the patient has every month been subject to an abscess in the left ear, from which a sanguineous puss was discharged. During the whole of the twenty years, her breasts were gorged with milk. She died in the fifty-first year of her age, in consequence of inflammation spreading from the ear to the brain. M. Russel and another medical gentleman examined the abdomen, where they found every organ in a state of integrity, but a considerable quantity of fat in the omentum.

Johnson, Sept. 1824.

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2. *Simulated diseases.**—Mr. Hutchinson has related a great many curious instances of feigned diseases among our seamen during the late protracted war, his situation, as a surgeon of the Royal Naval Hospital at Deal, having afforded him ample opportunities of witnessing, and often detecting, such impostures. We shall glance at some of these.

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* Mr. Hutchinson, Med. and Phys. Journal, Feb. 1824.

1*st. Ulcers.*—These have been formed and kept up by first incising a piece of skin, and then applying a blister, quick lime, mineral acids, &c. In a limb which Mr. H. amputated, he found a piece of copper coin imbedded between the gastrocnemius and soleus muscles. The man confessed that he had thrust this substance into the ulcer, about nine months previously! The usual security against such tricks, is a well-applied bandage, from the toes to the knee, sealed with wax; but this was found inefficient, as pins were thrust through it, and the ulcer irritated. A wooden box was then placed round the limb, which proved effectual in preventing such practices.

2*d. Diarrhœa.*—Our author has known diarrhœa, and even dysentery, induced in hospitals, for the purpose of invaliding—sometimes destroying the life of the infatuated self-tormentor. The means most resorted to by our seamen, were mixtures of vinegar and burnt cork, by which some of the finest young men in the service destroyed themselves.

3*d. Fever.*—This is a disease which Dr. H. has known to be feigned; he relates a case where a French prisoner swallowed tobacco, and covered his tongue with soap. The tobacco caused great rapidity of the pulse, but the matters ejected from the stomach smelled so strongly of tobacco that the imposition was soon detected, and confession followed.

4*th. Contraction of hands, elbows, knees, &c.*—These are very frequently feigned by sailors. They can only be detected by carefully attending to a variety of circumstances, and taking the impostors off their guard.

5*th. Opthalmia* has been produced by putting irritating substances into the eyes, as alum, lime, tobacco, &c.

6*th. Incontinence of urine.*—The best mode of detecting this imposture, in our author's experience, was the exhibition of laudanum, unknown to the person, and, when fast asleep, to place a clean napkin under him. The napkin, of course, will remain dry during sleep, if the disease be feigned; but be soiled, if real.

7*th. Strictures.*—This disease was often feigned by naval officers, when they wanted to leave their ships, from any cause of dislike to the captain or others. Mr. H. generally detected it by placing the person against a wall, so as not to admit of retreat. A bougie was then passed into the urethra, when, in many cases, much difficulty was experienced in getting it beyond the perineum. If suspicion of imposture be entertained, the mind of the patient is to be drawn off, if possible, and an opportunity taken of passing the bougie into the bladder. Are we not liable to some error here: when spasmodic stricture exists, which may allow the bougie to pass occasionally, though it may be a very troublesome complaint to the patient in general?

8*th. Hernia.*—Mr. Hutchinson remarks that, although seamen are very liable to ruptures from their great exertions, in furling sails, heaving up anchors, and working the great guns, yet the operation for strangulated hernia is very seldom necessary. This we know to be the case; but then we think the same remark extends to private life. There is not, we think, one case in five thousand which requires the operation, throughout England. Seamen, too, have immediate medical assistance, when an accident happens, which, of course, diminishes the chance of serious results. Mr. H. concludes this paper with a curious instance of voluntary power over the cremaster muscles, possessed by a man who could draw the testicles up into the groin, so as to produce the appearance of hernia there, and afterwards let them drop down to the bottom of the scrotum, when the examination was over. Mr. Hutchinson was, however, too lynx-eyed an observer to be deceived by this; for although the tumours in the groin puzzled him at first

sight, he soon proved an alibi of the testicles from their proper domicile in the scrotum, and saw them peeping through the pope's eyes. The man, on being detected, acted like a philosopher, and seeing no longer any chance of eluding the king's service, displayed before us several very remarkable feats of power he possessed over these organs.

Johnson's Journal, Sept. 1824.

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3. *Christison on the detection of minute quantities of Arsenic in mixed fluids.*—The object of this paper is to estimate the value of the liquid tests for arsenic, when dissolved in mixed vegetable and animal fluids, and of the various processes which have been proposed for correcting the changes thus produced in the action of the tests; and, finally, to determine what mode of analysis is, at the same time, the simplest, and may be applied under all circumstances. The author has extended the researches of orfila, and those on the modifications caused in the action of the tests by the coexistence of animal and vegetable fluids; and shows, by the instances of broth, tea, coffee, porter, port-wine, and milk, that, when the fluid is complicated, or deeply coloured, and the arsenical solution of such moderate strength as may be usually looked for in medico-legal investigations, the four best liquid tests—namely, lime-water, the ammoniacal sulphate of copper, the ammoniacal nitrate of silver, and sulphuretted hydrogen, are almost or absolutely useless. He next proves that no advantage is to be derived from the plans which have been proposed for restoring the true action of the tests, by destroying the colour of the fluids. These are two in number: the one advanced by Orfilal, in 1821; the other by Mr. Phillips, in January last. The former chemist proposes to destroy the colours by chlorine; the latter by digestion with animal charcoal. But Dr. Christison finds that, after the action of chlorine, the colour is seldom altogether or sufficiently destroyed, that the process does not take from the fluid the power it often possesses of retaining the arsenical precipitate in solution, and that, in some decolourised fluids, containing no arsenic, some of the tests cause precipitates very similar to those produced in pure arsenical solutions. He likewise finds, that the process by digestion in charcoal is sufficient, because, if the solution is not very strong, the charcoal removes all or nearly all the arsenic, as well as the colouring matter, and, if the solution be strong, it does not always lose its property of retaining the arsenical precipitates dissolved. He then proceeds to examine the processes which have been recommended by Rose and Rapp, for discovering arsenic, when intimately mingled with the animal textures, and which might also be applied to the residue by evaporation of mixed fluids, in which the common tests do not act characteristically. These processes it is unnecessary to mention. They are founded on the possibility of arsenic so combining with animal matters, as to resist the solvent power of boiling water. But Dr. Christison has found that, after careful digestion in water, no arsenic can be detected by either process; and, farther, that the process of Rapp is otherwise insufficient, when the quantity of arsenic is small, in which circumstance alone the ordinary means of analysis are inadequate. In the last place, he describes the method which he has found easiest of execution, and most generally applicable for detecting arsenic dissolved in mixed fluids, or mingled with such solids as are incapable of forming with it an insoluble compound. Minute directions are given, for the sake of the inexperienced; but we shall notice only the essential parts, and the result of his experience with respect to the delicacy of the method. Having observed that the sulphuretted hydrogen, though it seldom acts characteristically on diluted solutions of arsenic in mixed fluids, nevertheless, always throws it down of

some colour or other, even when the proportion of poison does not exceed an eight thousandth part, he proposes to employ this test with the view of procuring the arsenic in a convenient form for being subjected to the decisive process of reduction.

"The suspected matter, if solid, is to be divided into minute fragments, and boiled briskly in two or three successive portions of pure water. The fluid, whether originally such, or procured by digestion from the solid matter, is then to be subjected in a deep, narrow glass, for half an hour, to a brisk stream of sulphuretted hydrogen gas. In many cases, however, it will be necessary to premise the two following preparatory steps, before transmitting the gas; and, as we can seldom know before-hand whether these steps are requisite or not, it may be right to resort to them in every case. The first precaution is to add a little acetic acid to the fluid. By so doing, the influence of any free alkali that may exist in it is counteracted; and several organic principles, which might impede the subsequent separation of the precipitate, are coagulated. The second precaution is to boil the fluid for a few minutes; by which means some matters are separated, that the acetic acid could not throw down entirely; and any carbonic acid existing in it is driven off. The presence of carbonic acid in considerable quantity, by impeding the solution of the sulphuretted hydrogen, prevents its action on the arsenic, if the proportion of arsenic be small. The fluid is then to be filtered.

"When the stream has been continued a sufficient length of time, there is either a precipitate formed, or the fluid acquires a yellowish milkiness, which passes to a distinct precipitate as soon as the excess of sulphuretted hydrogen is driven off by heat. It is always right to boil before attempting to separate the matter thrown down; as the precipitate then becomes much more distinct, and falls to the bottom more readily. When the filtration is finished, and the filter has been gently compressed between several folds of bilbulous paper, the precipitate is to be scraped off with a knife, and dried on a bit of smooth paper, at a temperature somewhat above 212°."

The most advisable mode of subjecting this to the test of reduction is the following:—The best flux is the black flux, and the best instrument a glass tube about three inches long, and varying from a fourth to an eighth of an inch in diameter, according to the bulk of the material, which should not fill above three-fourths of an inch of the tube. The best mode of applying heat is by the alcohol lamp, as recommended by Mr. Phillips. "The true arsenical crust is known by the following physical character: Its outer surface next the tube exactly resembles highly polished steel. Its inner surface (which is best seen by scratching the tube with a file at the lower margin of the crust, and snapping it across,) is precisely like the fracture of fine steel, if the quantity is considerable: if it is minute, it has a dull bluish-gray appearance, but, before a microscope of four or five powers, appears brilliant and crystalline, like the fracture of steel. Occasionally, when very minute in quantity, it appears botroyoidal, and not brilliant, even before the microscope: in that case, the part of the tube to which it is attached should be coarsely powdered, and heated anew in a tube of less diameter. It is scarcely possible for any one to mistake these characters, particularly if he has ever seen an arsenical crust before; but, to prevent all possibility of error, the analysis may be concluded with the following experiment:—The part of the tube to which the crust is attached, being broken into fragments, is to be left for some hours in a watch-glass, containing a dilute solution of the ammoniacal sulphate of copper, and covered to prevent evaporation. In four or five hours, the metallic crust will become grass-green, or if very minute, it will be discoloured, and a brilliant grass-green crust formed on the surface of the liquor. The simple evaporation

of the fluid will cause the formation of a crust on its surface, though no arsenic be immersed in it. But in that case its colour is pale blue.*"

The author concludes by stating that the evidence thus procured is quite unimpeachable: that the process is the most convenient yet proposed; that it is probably applicable to all cases without exception, as he has found it to answer with the most complicated fluids he could select,—namely, broth, tea with cream and sugar, coffee similarly made, porter, port wine, and milk; and that it is sufficiently delicate for all medico-legal purposes, as it will detect satisfactorily a-quarter of a grain of arsenic, dissolved in 8000 parts of any of the foregoing fluids.—*Lond. Med. and Phys. Journ. Dec.* 1824.

4. *Bad effects of Ergot.*—M. Gerardin lately read to the Academie Royale de Medecine, an account of certain deleterious consequences, resulting from ergot, when employed to facilitate labour:—he stated, that in the colonies, this substance is looked upon as a sure means of producing abortion, and destroying the life of the fœtus in utero:—he therefore is of opinion, that it ought not to be administered without the greatest caution, and that it ought only to be employed in certain cases of want of action in the uterus during labour, or to expel hydatiform masses developed in its cavity.—*Revue Medicale, Sept.* 1824.

5. *Common Salt to correct the Fœtor of Cancerous Sores.*—M. Siaboun relates the case of a female with a cancerous ulcer in the right breast, who suffered extremely from the fœtor of the discharge, which was so intolerable, that the persons who assisted in dressing the wound, could scarcely remain in the room. M. Siaboun ordered a lotion of salt, dissolved in water, to wash the parts: the disease was not arrested by these means, but the fœtor was entirely destroyed.—*Journal Medicale de la Gironde.*

6. *Effect of Counter-irritation.*—Mr. Ogden has related, in the Medical and Physical Journal, a case illustrative of the effect of extensive counter-irritation. The patient was a child, six years old, who had been affected with cough and copious expectoration from the earliest period of life. During the last two years he had also been subject to epileptic fits, recurring at intervals of nine or ten weeks. But these were always brought on, or preceded by a deranged state of the alimentary canal. On the twenty-second of October, 1821, the child's clothes took fire and he was dreadfully burned—the skin being removed from the whole of the thorax in front, and a considerable portion of the abdomen—added to this, the turpentine dressing produced an extensive erythema of the skin. In consequence of these irritations, the pectoral and epileptic complaint disappeared.

7. *Cases of Chronic Cephalalgia.* By Lewis Frank, M. D. &c.—A number of physical and moral causes, says Dr. Frank, may produce chronic cephalalgia. Although many of these are readily apparent, yet cases occur in which they are by no means manifest, or are of such a nature as not readily to suggest themselves to the mind of the practitioner. In many of such cases, therefore, the treatment cannot proceed upon pathalogical principles,—the physician must then have recourse to a rational empiricism. Without being new, the following cases may be recollected with advantage:——

* If portions of the crust are simply exposed to the air, they soon acquire a grayish-black colour on the surface, a character also indicative of metallic arsenic.

Case 1.—When at Newstadt, near Vienna, in 1815, I was informed that a woman who had been distressed with severe cephalalgia, and obliged to keep her bed during seven years in consequence of the violence of the pain, was cured, as if by enchantment, after the expulsion from the nose of about fifty worms, accompanied with mucus. Wishing to ascertain the truth of this statement, I visited the woman, when she confirmed the fact, and added, that she had consulted, previous to this occurrence, several physicians, and employed without benefit a great variety of remedies. Despairing of relief, anxious to hazard every means, and observing that her mother was in the habit of giving her brothers and sisters a powder consisting of the semen contra, (artemisia santonica, worm seed) valerian, and assafœdita, as a vermifuge, she formed the resolution of taking it in the form of snuff. This powder occasioned great sneezing, and the evacuation of much mucus, followed by several living worms. She continued her remedy, and in the space of about a month she discharged from the nose the number of worms already mentioned. She was completely cured of her complaints, returned to her occupations, and now, four years after the occurrence, remains well. The noise which this circumstance occasioned in the village, induced all who have since been affected with head-ache to resort to this powder for relief, and many have actually derived advantage from the use of it.

Case 2.—In 1818, when visiting the Hospital of Incurables of Yps, in Upper Austria, the physician of that institution showed me a female who had been tormented for several years by violent head-ache, occupying chiefly the forehead and vertex. She had discharged a number of worms from the nose, having the form of small lumbrici, or larvæ of considerable size. Several of these worms were preserved in spirits, and sent to Bremser at Vienna; but this celebrated helmenthologist could not be precise respecting the species, as the too-concentrated spirit which had been employed for the purpose of preserving them had altered their structure. The patient had been treated by a solution of *oxymuriate* of mercury and valerian. She assured me, that each time she discharged these worms, her head-ache was relieved for a time.

Case 3.—A female, forty-six years of age, came to consult me at Parma, for severe cephalalgia, which had continued four years. The pain occupied the whole head, and seemed to be seated in the pericranium (rheumatic cephalalgia.) She had been treated by several physicians without having derived benefit. This patient had the appearance of health, and the natural functions presented no disturbance. I was embarrassed as to what treatment I should adopt. Believing the disorder, however, to be of a rheumatic nature, I prescribed a powder consisting of two grains of the sub-muriate of mercury, and a grain of the yellow hydro-sulphate of antimony, with a little sugar, to be taken twice daily. A year afterwards, I saw this patient, who informed me that she had been completely cured of her head-ache after having used this powder for a month.

Case 4.—A goldsmith, at Verona, in 1822, of a good constitution, aged forty-four, consulted me on account of a violent cephalalgia, obliging him sometimes to keep his bed, and producing vomiting. I supposed that the disease was owing to the mercurial and arsenical vapours, and therefore decided on prescribing the same powder that I had administered to the subject of the third case, with the addition of the infusion of *artemisia virescens* every morning. Passing through Verona, in 1823, this goldsmith called on me, and informed me that he had experienced no cephalalgia during the eight preceding months. He stated, that a few days after he had commenced the use of his medicines, he had passed by stool a great number of large lumbrici. It may be remarked, that the *artemisia virescens* possesses greater anthelmintic virtues than any other bitter substance.

Case 5.—A lady, aged forty-five, of a delicate habit, and pale, complained of violent cephalalgia, in paroxysms of three or four hours duration. Conceiving that it might originate from the state of the abdominal viscera, I administered an emetic and gentle purgatives, but without advantage having been derived from them. Valerian, cinchona, &c. were also prescribed, with the same result. The patient having remarked, that the presence of cold water in her mouth gave relief, I was induced to examine her teeth: the molares were affected with caries. The affected teeth were removed, and the cephalalgia disappeared without further treatment.

Annali de Medicina, June, 1824.

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8. *On the method of counteracting Uterine Hæmorrhage, proposed by* Dr. Gooch.—Since the publication of Dr. Gooch's paper, (12th vol. Medico-Chirurgical Transactions,) on the hæmorrhage which sometimes follows the expulsion of the placenta, and the contraction of the uterus, I have had an opportunity, (says Mr. Crowfoot,) in three cases, of witnessing the good effects of the preventive treatment which he recommends.

Mrs. G——, in three previous labours, had suffered from a frightful hæmorrhage, which followed the expulsion of the placenta, after an interval, varying in the different labours, of from five to twenty minutes, notwithstanding the uterus had actively contracted. This patient having been again pregnant, I determined to try strict antiplogistic treatment for five or six weeks prior to her confinement. The consequence was, that she altogether escaped the hæmorrhage.

Mrs. A——, a lady of sanguine temperament, but delicate constitution, in her two first labours, had rather more than the usual discharge after the removal of the placenta. In her third labour, the placenta was expelled as usual, the uterus actively contracted, and no hæmorrhage followed for at least twenty minutes, when a most apalling one burst forth, and was with difficulty restrained by the most prompt and continued treatment. In her fourth pregnancy, the antiphlogistic treatment was pursued as in the former case, and with the same satisfactory result.

In another patient, Mrs. L——, similar hæmorrhages had occurred in two previous labours: in a third pregnancy, the antiphlogistic treatment prevented this inconvenience in the following labour.

In none of these cases did the hæmorrhage immediately follow the expulsion of the placenta; an interval of from five to twenty minutes intervened; and I therefore infer, that a degree of relaxation of the uterus preceded the hæmorrhage, and was as essential as the existence of the phlogistic diathesis. We find that the only remedies upon which we can depend in these cases, are such as have a tendency to produce firmer contraction of the organ. The sudden application of cold to the abdomen, steady pressure externally on the uterus, and the introduction of the hand into its cavity. *Edinburgh Med. and Surg. Journal.*

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9. *Effects of castration in certain animals.*—M. Fanneau Delacour, of Souzay, has performed a number of experiments upon sheep and chickens, with a view of determining the effect of castration upon the animal economy, conceiving that the loss of organs so important as the testicles, could not take place without materially affecting the health; which opinion was strengthened by considering the sudden evils often arising from more trifling causes,—such as the disapearance of eruptions, or the drying up of long-established ulcers. M. Delacour had eighty chickens castrated in his presence. Eleven of these exhibited well marked signs of cerebral affection, and in three others the symptoms were observable, but not to so great a degree. Of eight which became mad, four of the worst, as well as

two out of three which were threatened with apoplexy, were cupped upon the rump, and an actual cautery applied on each side of the cupping glass; and, in the four first instances, a cautery was also applied on the head. All these recovered; one left entirely to the efforts of nature, died on the third day—the brain exhibiting the strongest marks of inflammation. The same phenomena were observable among a flock of sheep, and in a greater proportion. The same remedies were made use of in seven of these animals, and they all recovered: whereas, two, left to nature, died; one on the fourth day, with all the marks of madness; the other, on the second day, in a state of coma. The examination of the heads, showed, in the first instance, a violent state of inflammation of the brain and its membranes; the brain of the second was softened, and the ventricles filled with a fluid resembling the white of an egg a little coloured.—*Journal Universal des Sicences Medicales.*

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10. *Hydrocyanic acid in Tænia.*—M. Galneche of Stetin, records the following case. A boy, three and a-half years old, who was afflicted with the *tænia lata*, was recommended to eat strawberries for two days in as great quantities as he pleased, the author observing that when the presence of this worm is suspected, the strawberry generally produces a discharge of some few portions. The third, at six in the morning, ℥ss. of castor-oil was given; at half-past six, at seven, and at half-past seven, he took each time fifteen grs. of the powder of male fern root: and at eight o'clok, six drachms of castor-oil. At half past eight an abundant serous motion took place, and a quarter of an ell of the worm escaped from the anus; the little patient was then placed upon warm water, into which the worm was suffered to be suspended; it was slightly laid hold of at the orifice of the anus, and rubbed over for the space of four inches, with hydrocyanic acid. It immediately made strong efforts to return to the abdomen, so that it was obliged to be held with some degree of force; it suffered some convulsive motions, and was expelled the length of an ell and a half. At the termination of an hour and a half another evacuation produced the remainder of the worm, two ells long; the animal was dead, it terminated in a threadlike extremity, surmounted by a reddish head of the size of a millet seed, armed with a little sucker.—*Journal der Praktichen Heilhunde.*

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11. *Martinet on the Nerves.*—According to this author, inflammation of the nerves is a cause of neuralgia, but among the least frequent. This inflammation has its seat in the neurilima, &c. in the cellular tissue which unites the nervous filaments together. It has for a constant mark the development and increase of pain on pressing the affected nerve. This inflammation differs from neuralgia proper, which consists solely in the alteration of the sensibility of the medullary substance, in which the pain is not always agravated by pressure, and in which it is always subject to remissions.—*Lond. Med. and Phys. Jour.*

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12. *Miningene.*—The farther we see into the animal economy, the more we shall be convinced of the intimate sympathy between the alimentary tube and the sensorium. Dr. Scoultor of Mentz has published, in the *Journal Universal des Sciences Medicales*, some anatomo-pathological researches on this subject, for the purpose of illustrating the connexion between irritation of the mucous membrane of the intestines and the pia mater. He thinks that diseased states of the latter delicate membrane have been often overlooked, in consequence of the changes in its structure not being very conspicuous to the hasty and superficial observer. We conceive, however, that the changes of structure in this tissue are more easily detected than those in most other parts of the body. In a healthy state, this web is per-

fectly transparent, thin, and colourless, and without adhesions. Any deviation from this state, therefore, must necessarily be one of disease. It is no doubt desirable, but seldom practicable, to have a healthy brain to contrast with an unsound one prosecuting *post mortem* researches. This was the plan pursued by Wenzel in examining the brains of epileptic patients. Dr. S. thinks that all parts of the intestinal canal do not equally sympathise with the brain. The stomach and small intestines possess a much closer affinity with the meninges than the great intestines. While inflammation of the mucous membrane gives rise to corresponding disturbance in the *pia mater*, this last remains free from all sympathy in peritonics. When the mucous membrane of the stomach or small intestines is inflamed, Dr. S. has observed the vessels of the *pia mater* injected with blood, forming red patches, and sometimes amounting to extravasation. These appearances are chiefly found in the anterior and lateral parts of the brain—unless the abdominal inflammation be very violent; in which case nearly the whole of the *pia mater* is affected, its minute vessels carrying red blood and giving rise to effusion and adhesions. Dr. S. thinks that these views are likely to bear and elucidate many of the phenomena of apoplexy, on the principle that every inordinate excitement of the stomach being communicated to, and, as it were, repeated in the brain; thus occasioning determinations of blood to the head, and rendering its vessels gradually more debilitated, and ready to give way under any increase of action.—*Med. Chirurg. Rev.*

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13. *M. Savart on the Ear.*—M. Savart has published the results of numerous experiments, made with a view of ascertaining the uses of the membrana tympani and internal parts of the ear. Referring to the paper itself for the details,* we must content ourselves with enumerating the conclusions at which he has arrived. They are—1st. That the communication of vibrations by means of air appears to observe the same laws, at least with respect to minute distances, as those which govern solids. 2d. It is not necessary to suppose, as has been done heretofore, the existence of a particular mechanism to make the membrana tympani always vibrate in unison with the bodies which act upon it. 3d. That the degree of tension does not vary, except to augment or diminish the amplitude of its excursions (*excursions,*) as supposed by Bichat; but with this difference, that M. Savart's experiments prove the reverse of Bichat's conjecture, which was, that the membrane was relaxed by powerful impressions, and became tense from those which were feeble. 4th. That the vibrations of the membrane are communicated without alteration to the labyrinth, by the medium of the ossicula, as the vibrations of the upper table of a musical instrument are transmitted to the lower. 5th. That the ossicula likewise modify the extent of movement in the vibrating parts contained in the labyrinth. The author further thinks that the extent of the membrane of the tympanum in different animals influences the number of sounds which they can perceive, and likewise the limits at which these sounds commence, or cease to be perceptible to the animal.—*Lond. Med. and Phys. Jour. Jan.* 1825.

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14. *M. Mondini on the Eye.*—A paper of some interest has been published on the Eye, by M. Mondini, of Bologna. It relates chiefly to the pigmentum nigrum, which he asserts to be properly no pigment, but a true membrane. The black colour, he affirms, is owing to the presence of minute particles of oxid of iron, which may be separated by the loadstone.† *Ibid.*

* *Journal de Physiologie*, No. 2, 1824.

† *Oper. Scient. dell Univer. de Bologna.*

15. *Dr. Knox on Comparative Anatomy.*---Dr. Knox, whose researches in comparative anatomy have procured for him a place among the most industrious naturalists of the day, has published some remarks on the lacteal system in the testaceous mammalia, which are of importance, both as they disprove a position of the celebrated Tiedeman, and as they bring us back to an old doctrine, which had been invalidated by the assertions of the naturalist above mentioned. Tiedeman had been led to suppose that, in some animals, as the seal, the chyle having been conveyed to the mesenteric glands by the common lacteals, (viz. the vasa afferentia,) was there absorbed by veins only, and thus conveyed directly into the blood, without passing along the thoracic duct; and, consequently, that this duct was destined, in all such animals, for the transmission of lymph alone. Dr. Knox has succeeded in demonstrating the vasa *efferentia* as well as *afferentia*, and showing, by a preparation deposited in the museum of the College of Surgeons in Edinburgh, that they may be injected with quicksilver. Thus, we return to the established opinion, that there exists, in all animals with which we are acquainted, a peculiar set of vessels, whose proper business it is to convey into the blood the nutrient fluid derived from the food.*—*Ibid.*

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16. *Dr. Francini on Absorption.*—Dr. Francini has performed a series of experimental investigations relative to Absorption, of which the following are the most important results:—1. That the lymphatic vessels of the intestines are those which absorb the chyle. 2. That it is not demonstrated that they absorb any other fluid in the intestinal cavity. 3. That there is no solid argument tending to prove that the lymphatics of other parts are absorbents. 4. That it is certain that the veins of the intestines, of the abdomen, and bronchiæ, absorb. 5. That experiment has not made it obvious whether the lymphatics or veins absorb the fluids existing in the cavities of the cellular tissue, &c. 6. That, from numerous experiments, both in the state of health and disease, it is probable that the absorption of fluids in the cavities is, in a great measure, performed by sanguiferous vessels.†—*Ibid.*

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17. *M. St. Vincent on Generation.*—The subject of Generation has recently received considerable attention, if not much elucidation, from several physiologists. The observations to which we allude are principally directed to the spermatic animalculæ. The existence of these is asserted, by M. St. Vincent, to admit of ready demonstration, by taking the testicle of an animal recently dead, and making a puncture in it; the semen oozes out, and a small portion of it is to be examined through a microscope. The animalcules are so numerous, that they scarcely have room to move; but, if the semen be diluted with water, they become separated, and are more easily subjected to scrutiny. Their resemblance to tadpoles is said to be striking. Various other particulars are detailed in the paper from which these remarks are taken.‡

MM. Prevost and Dumas have likewise instituted experiments connected with this curious subject.§ They deny the influence of the aura seminalis, deem the spermatic animalculæ necessary, and hold that the semen must be actually introduced into the uterus in order to produce impregnation. If this be so, there are some anomalies to be explained.—*Ibid.*

* *Observations on the Anatomy of the Lacteal System in the Seal and Cetacea;* by Dr. Knox, &c. Edinburgh Med. and Surg. Journal, July, 1824

† *Opuscol. Scient. di. Bologna,* No. 1.

‡ *Journal de Physiologie.*

§ *Dictionaire de Medicine,* art. Generation.

18. *Utility of Fresh Vegetables in Fever, when placed near the Patient.*—We have received a letter from Mr. S. Shatte, of Stockenham, Devon, the purport of which is to call to our notice a practice formerly recommended by some of the ancient writers on medicine, and which, he says, he has found of great service in fevers of the typhoid kind. A short extract from our venerable correspondent's letter will explain his meaning sufficiently.

"In 1821, when typhus fever was very prevalent in this neighbourhood, I made several trials of this remedy. G. F. in the village of Chillington, was taken ill with that disease, having a wife and two children, with only two small rooms up stairs; I immediately had the rooms and the bed covered with wet bushes or boughs of ash, hazle, willow, or any green shrubs that could be procured. The old ones were carried out, and fresh ones brought in every morning: and I am of opinion that, when they are brought in with the dew upon them, they are more efficacious: at all events, they must be made very wet with cold water, and be in considerable quantities, so as to cover the whole room."

In one case, our correspondent adds that they appeared to revive a patient almost at the point of death.

Mr. S. adds several quotations from Fernelius, Nic. Fontano, &c. who advocate the same practice; and finally he objects, and we think with great reason, to the abstraction of large quantities of blood in pure fever likely to assume a typhoid character.—*Ibid.*

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19. *Dr. Kinglake on Hernia.*—The mode of rendering the assistance, which it is the object of this paper to recommend, is that which has succeeded in six instances out of seven, and that too within ten minutes after commencing the trial. The principle of the treatment is founded on a leading and characteristic feature of the disease and of the practical difficulty with which its remedial means are closely connected: it is that of the contained air in the protruded intestine inflating and distending the prolapsed part to an extent that renders it too bulky to admit of its being returned through the unyielding or undilatable opening by which it has passed. What therefore should be attempted? Most certainly to diminish the flatulent distention or bulk as much as possible, so as to bring it within the area of the opening through which it is to be replaced.

This great remedial object may be effected by reduced temperature, topically applied through the medium of the coldest water that can be procured. Its sudden application by means of cloths dipped into that fluid, and renewed every two or three minutes during about half an hour will have the effect of so far abstracting heat from the hernial parts as to cause a perceptible diminution of the swelling. This is produced by the condensing influence of cold on the rarefied state of the air contained in the prolapsed portion of intestine.

When this diminution has been produced, the endeavour should be commenced to return the hernial tumor, and the mode of attemping it should be by pressing steadily but forcibly at the basis of the swelling, with a view to insinuate, as it were, a small portion of the air lying nearest to the hernial aperture in the first instance. This incipient step to ultimate success may with suitable management be soon effected, when the remainder will follow with the containing portion of intestine, and in passing the aperture will enter the cavity of the abdomen often with an audible report, and always with a jerk, not less satisfactory to the practitioner than gratifying to the patient. In all the instances in which this mode of relief has been pursued, no subsequent inconvenience occurred; the bowels soon became, with the aid of aperient medicine, sufficiently active; the previous vomiting ceased; and complete recovery was the happy result.—*Ibid.*

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20. *Balfour on Hernia.*—In the month of August, 1814, I was called to Mr. Samuel Malcolm, aged twenty-eight, whom I found labouring under

symptoms of strangulated scrotal hernia. He had worn a truss for several years, but, the strap having broke, had laid it aside for two days previous to that on which I was called to him; when a hernial tumour, of considerable size, was formed, and refused to recede by the means he usually employed on similar occasions. I found him in bed, on account of a chilliness of which he complained, and general tenderness of the tumour to the touch; pulse natural.

I put my patient in the posture recommended by Mr. Samuel Cooper, and then proceeded according to Mr. Hey's directions, "by gently compressing the neck of the hernial sac, that its bulk might be diminished where the stricture was the greatest; and then pressing the diminished part towards the abdomen." I continued my exertions, with short intervals, for fifteen or twenty minutes, without effect. The patient had now become thirsty, yawned incessantly; and the tenderness of the parts had increased so that he intreated me to desist.

Finding I could not succeed, and satisfied that the tumour was exclusively intestinal, it occurred to me that, if I could pull the protruded parts downwards a little, inflammation would be retarded, by the constriction being removed to another portion of the bowels; that both constricting and constricted surfaces would in some degree be lubricated; that the distention of the vessel of the incarcerated portion of the gut would be taken off, and consequently its whole bulk diminished; and that the peristaltic motion might be restored: effects, which would all conspire in facilitating reduction.

But is there no injury to be apprehended, I asked myself, from using such freedom with the parts? If resistance, I replied, is considerable, I will desist; if there is little or no obstruction to their further descent, I can do no harm: for inflammation, however imminent, has not yet commenced. Besides, if Mr. Benjamin Bell and Mr. Hey could suspend the whole weight of the intestines by the protruded part without injury, much less risk must be run in applying a power which can be regulated at pleasure. Supporting the scrotum, therefore, in my right hand, and doubling up the integuments with the finger and thumb of my left, I laid hold of the gut, and pulled it gently downwards. Immediately on feeling it yield, I applied pressure upwards as before; when the tumour receded into the abdomen in the pleasantest manner, and with a gurgling noise.

From the date it will be seen that the above case occurred ten years ago. Indeed it had entirely escaped my memory, till, rummaging one day among a parcel of manuscripts, it presented itself to my view. I now lost no time in taking the opinion of some of the most eminent practitioners in this city on the subject. Every one stared at mention of the fact, as if he would have said, "How simple the idea! It is astonishing it did not occur to myself." All agreed that it was new, and that it unquestionably superseded the necessity of having recourse to the knife in the present instance. Now, any mode of cure that succeeds in one case may succeed in others similar to it.

As illustrative of the probable advantage to be derived from the fact in question, I would suggest a consideration of the phenomena which primarily occur in hernia. What, for instance, must be the immediate effect of even the slightest circular compression of the intestinal tube? Proportionate distention, certainly, of the vessels below the stricture, or of the isolated portion of the gut. It is very conceivable that cases of hernia frequently occur, in which the protruded parts completely fill the aperture through which they pass, without being sensibly constricted for a considerable length of time; and yet distention of the vessels below the aperture gradually, though imperceptibly, advances to that pitch at which it forms an insurmountable obstacle to the retrocession of the gut. A very slight degree of such distenton may effectually preclude success in the operation of taxis. But if, in such circumstances, the stricture can be shifted, as in the above case, to a different portion of the tube, the necessary consequence would be a diminution of distention of the vessels of the isolated portion. If to these are added

the other considerations which determined my practice in the present case, they will form a powerful *a priori* argument for its adoption in many cases of daily occurrence. At the same time, I am well aware that the utility of any new fact or practice in medicine and surgery can only be determined by time and experience.—*Ibid.*

21. *Dr. Hull on Hernia.*—About 10 months ago, the attention of one of the editors of this Journal was called to Dr. Hull's new patent hinge truss by the following circumstance: An eminent merchant from Charleston had been under our care, during a few weeks, for an affection of his head. On taking leave he observed, that he thought it a duty which he owed to the interests of humanity to make known to us the great advantages he had experienced from the use of "the New-York truss." He had been afflicted with an inguinal hernia several years, for which he had consulted many surgeons in this country and in Europe, and applied every kind of truss that could be obtained without being able to prevent the disease from returning on the slighest exertion. A few months before he came under our notice, he had consulted Dr. Hull, in New-York, who applied the new hinge truss with such perfect success that no descent of the hernia had ever afterwards occurred. The most interesting circumstance in the case, however, was, that no inconvenience had been experienced from the use of the instrument, which the gentleman recommended to us as by far the most excellent one that had ever been applied on his person. The warm and urgent recommendation of our patient induced us to procure a number of the instruments thus alluded to, for the purpose of giving them a fair trial under our own immediate observation. We have since that time applied them upon eight different patients, two of which were afflicted with double scrotal hernia. The results of our experience have been so satisfactory that we cannot avoid offering our testimony in favour of the new truss, which we consider as altogether the best which we have as yet been made acquainted with. We have lately been favoured by Dr. Hull with a copy of his pamphlet, and a bundle of impressions from his plate which illustrates the mode of applying the instrument. We will extract a few passages from the pamphlet, to accompany the plate on the opposite page. After pointing out the inconveniences and defects attending the use of the common kinds of trusses, he proceeds as follows:

"It is a matter of the deepest regret, that circumstances of so much consequence to an unfortunate class of our fellow-men should have been so little attended to in the construction and application of the preventive means for the different species of hernia: and that trusses combining so few of the principles which lead to the prevention of this calamity, and many of them evidently calculated to increase the difficulty and danger of the patient, should have obtained such general use, and been recommended in so many works of merit, which but tends to propagate error, and to prepetuate that obstinacy of prejudice for which the faculty are already but too justly distinguished.

"It was witnessing the above enumerated defects of all trusses, and the dreadful effects attendant upon rupture, which first led us to reflect upon the nature of the complaint, and to search for some more efficient means to mitigate the distress, and preserve the lives of our fellow-men. As a compensation for our research, is the gratification that we have thus far been enabled not only to secure every instance of reducible hernia, but to restore to perfect health a great proportion even of very aged sufferers. In the almost innumerable cases of children when this instrument has been applied, not a single instance has occurred, (within our knowledge,) which has not been cured in less than eighteen months, and few have exceeded six.

"The qualities we have united in the truss, are equally applicable to every species of hernia, and we can say, without the fear of contradiction,

that the proportion of cures it has effected is altogether unparalleled. It may, perhaps, be an interesting inquiry to some, how this instrument produces its effects; and we think, after considering its construction, this question can be answered to the satisfaction of every rational mind. It will be observed, that this truss presents a concave surface to the rupture opening. The concavity of the plate is occupied by an elastic cushion, the resistance of which is sufficient to reduce the protruding intestine while it is prevented escaping to any considerable distance by the pressure of the metallic plate. Which pressure being greatest at the circumference and diminishing toward the centre, tends constantly to approximate the hernial parietes and afford them rest, and mechanical support. It is therefore obvious that nothing is suffered to intervene between the lips of the opening, as is the case when the intestine protrudes, or a convex pad is applied; but a fair opportunity is presented for the fibres to recover their tone, or to heal; when any laceration has been produced by violence done to the parts. It is a law of the animal economy, particularly noticed by Dorsey, that all hollow parts of the body, have a tendency to adapt themselves to their contents.

"For the cure of hernia, then, it is only necessary to remove every obstacle which counteracts this tendency. This indication is certainly very far from being answered by the convex pad, and we think it can only be fulfilled by one which shall reduce the bowel without dilating the ring; with this view, we have applied the concave pad, which has more than answered our expectations, in preventing a descent of the gut, and in restoring the fibres, which it undoubtedly greatly facilitates by its constant and uniform pressure. But without investigating the *modus operandi*, it is sufficient for the patient, and for all practical purposes, for the physician to know, that with this instrument hernia may always be secured. If applied in cases of unbilical, or congenital hernia in children, it will in every instance remove the necessity of an operation. In cases of congenital hernia, it should be applied before adhesion takes place, but not until the testicle has made its descent. If this particular period should be more carefully observed by surgeons, and the application of the truss, (instead of being abandoned to mechanics,) receive a greater share of their attention, they might be instrumental in obviating much of the distress which has been entailed upon the world.

"It is a fact well known to the profession in the county of Oneida, and has been a subject of congratulation in their Medical Society, where the truss was invented and first brought into use, that from the year 1800 to 1819, from three to five persons were annually the subjects of operations from strangulated hernia; and that from the year 1819 to 1822, not a single case had occurred within the knowledge of any individual member of the society. This surprising alteration was attributed, solely, to the general use of the instrument now under consideration.

"H—represents the umbilical truss;

"A—the situation of the rupture pad in umbilical hernia,res ting with its centre directly over the rupture opening. The spring of the pad passes close to the side, as represented by the plate.

"D—represesents the situation of the back pad upon the centre of the spine in corpulent persons, but a little beyond on spare or emaciated.

"I—is a representation of the double truss, when upon the body the principal part of the pressure is made upon the end pad, C; that should, therefore, be applied to the worst rupture.

"G—represents the single ing. truss; it differs in no respect from the double, without the addition of the second pad. If the rupture be in the right groin, this truss comes on from the left side as represented by K. And the same truss reversed for a rupture in the left groin.

"Fig. F—represents the instrument as applied in femoral hernia. The curve in the spring at F, is for the purpose of dropping the rupture pad lower in the groin, while the back pad retians the position E."

JEFFERSON MEDICAL COLLEGE.

The Medical Faculty of *Jefferson College* was instituted in June last, by an act of the Trustees of that Institution, appointing Professors to teach the various branches of knowledge comprised in a regular Medical Education.

By the same act the Faculty of Medicine were authorised to locate themselves in any part of the State of Pennsylvania, affording, in their judgment, the greatest facilities for the discharge of the trust committed to them. In these circumstances, the Faculty could not long hesitate in selecting Philadelphia, as obviously presenting to the Medical Student advantages for improvement far greater than could be found in any other part of the state.

In order to carry into full execution the design of the Trustees, a large and convenient building has been procured, and fitted up in a manner as suitable as could be desired for the Lectures and Demonstrations, connected with a complete course of Medical instruction.

The Lectures will commence on the Thursday immediately preceding the first Monday in November next, and will continue until the last of February.—The commencement, or time of conferring degrees, will be as early in the month of March as the examination of the candidates will admit.

Those persons will be entitled to a degree, who shall have attended two full courses of lectures in this school, or one course in any other school which has the power of conferring Medical Degrees, and one in this Institution; and who shall have been found qualified for a degree on a suitable examination.

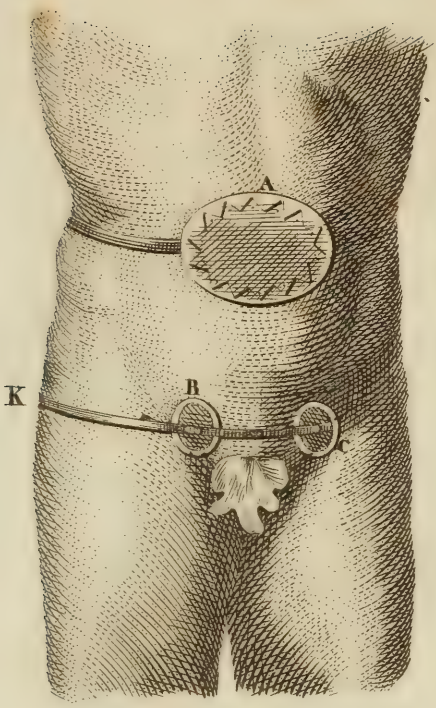
The fees for attendance on the Lectures will be $15, to each professor, for the course.
The fee for Graduation, $15.
No Entrance fee or matriculation ticket will be required.

JOHN EBERLE, M. D. Prof. Theor. & Pract.
GEO. M'CLELLAN, M. D. Prof. Surgery.
JACOB GREEN, A. M. Prof. Chemistry.
B. RUSH RHEES, M. D. Prof. Mat. Med.
F. S. BEATTIE, M. D. Prof. Inst. of Med. and Midwifery.
NATHAN R. SMITH, M. D. Prof. Anat. & Physiol.

B. RUSH RHEES,
Dean of the Medical Faculty.

An Infirmary will be attached to the Institution for the benefit of the poor, requiring Medical or Surgical aid; to which all the Students will have free access, during the hours of operating and prescribing.

Large and commodious apartments are provided for the purpose of Dissection.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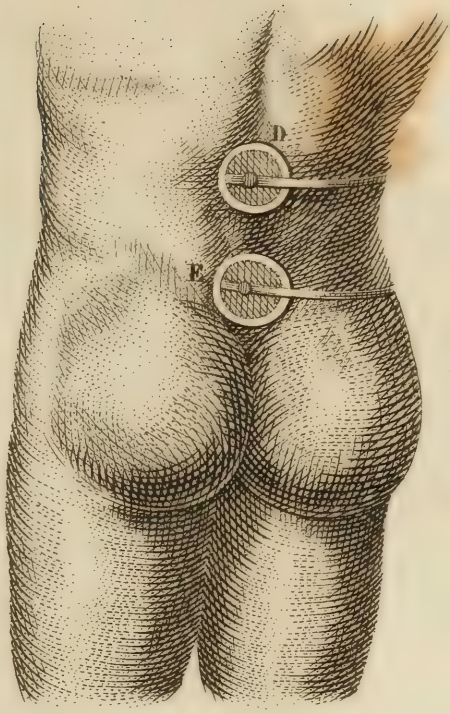
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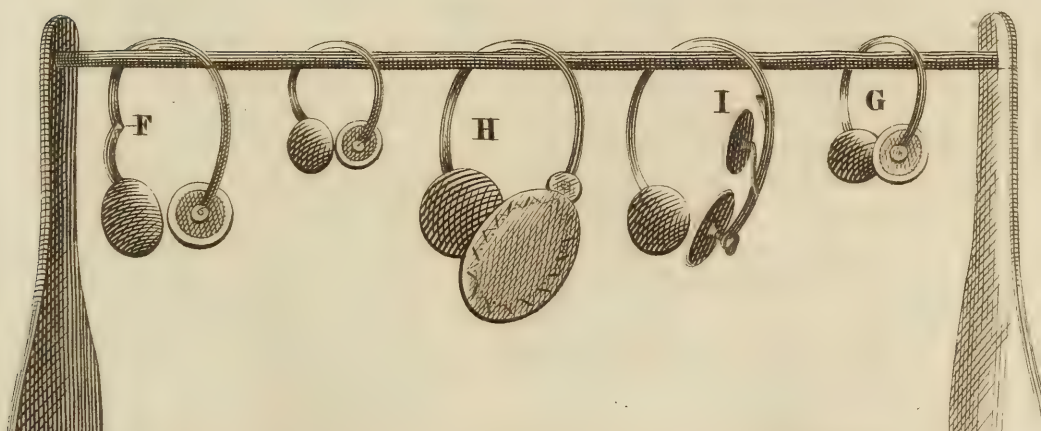
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